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Assyrian cherub pollinating a date palm. (Bas-relief from the palace at Korsabad, ca. 800 B.C., University Museum, University of Pennsylvania.)

THE EVOLUTION OF BIOLOGY

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Preface

In some respects this book may be said to be a war baby. During the years of the Nazi occupation of the Netherlands, many of the Dutch sought an antidote to relieve the tensions caused by the strain of living in an occupied country. The senior author spent hours of refuge in writing a sketch of the development of biology (*De ontwikkeling der biologie*, Gorinchem, Noorduyn en Zoon, 1942; Second Edition 1947). A few years later it was decided to prepare an American version developed into a more complete survey of the evolution of biology.

To aid in this, the junior author undertook the task of writing introductory chapters on the prehistory of biology and on some of its pre-Renaissance developments. The book has now become a survey of the evolution of biology from its beginnings before the dawn of history to its most recent and very spectacular advances.

The title of this book was chosen deliberately. It was never the intention of its authors to write a detailed history of biology. As the title indicates, their objective was to survey the developmental phases through which biology has passed in its long road toward becoming an independent discipline, and also to indicate the paths it traversed in attaining its present rank among the natural and physical sciences.

The authors thank Helen Kingsbury Zirkle for the careful preparation of the Index and Mr. L. Sprague de Camp, who read the manuscript and suggested a number of stylistic improvements. Thanks are also due Miss Rosa-Lida Polak, who assisted in translating portions of the text.

M. J. Sirks
Conway Zirkle

March, 1964

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The Beginnings of Biology

Biological knowledge is older than recorded history, and a moment's thought will show us that this is not as odd as it may seem. Ever since we evolved into human beings, and even before that, we have had to depend upon what we knew about animals and plants, not only for our safety but also for our food—in fact, for our very existence. A mistake in recognizing a fellow mammal or an error in identifying a poisonous plant might well prove fatal, and probably did prove fatal to many of our prehistoric uncles and aunts. Actually, our ancestors had to be familiar with a great many animals and plants if they were to survive the rigors of prehistoric life and evolve to a stage where they could live in comparative safety and satisfy their hunger with fair regularity.

As the ages passed, this painfully acquired knowledge accumulated until it reached a point where our progenitors had to systematize it; if they did not, they could not remember all that they had to know if they were to remain alive. It was the organization of this knowledge that brought the science of biology into being.

At first it was a very primitive and a purely empirical science. Its theoretical framework was often fantastic—when it was not lacking entirely. But it was a useful science and its application to the problems that faced our ancestors enabled them to survive a not too happy phase in the evolution of our species.

Compared with biology, history came on the scene quite late. In the cosmic time scale, history is definitely nouveau; and, as a science, it is still immature. History could make its debut only

after writing was invented and after a culture had developed to a point where it had to keep its records. All such literate cultures grew up in regions where the human population was concentrated —sometimes in cities—and we apply to the cultures that invented cities the adjective "civilized." We say that with the development of literate and urbane cultures, civilization came into being.

But civilizations cannot arise just anywhere or under just any condition. They have many needs that must be satisfied before they can be born. They require, first of all, that a great many people maintain a relatively close cultural contact, and this means that the population must be relatively dense, too dense in fact for any mere food gathering or hunting economy to support. All civilized communities need great quantities of food—quantities that only an effective and stable system of agriculture can supply. Thus agriculture had to be well advanced before civilization could start. We might say that it had to develop into an applied science. Agriculture actually had begun its development some ten thousand years before cities were built and long before anyone had learned how to write.

In its beginning, agriculture was limited and very crude. But, as the centuries passed, it improved, and the knowledge of agriculture accumulated. Even the more advanced agricultural systems, however, contained many irrational elements, and many of the common rules for growing crops were based on formulas that had developed out of common superstitions and earlier magical practices. Even today the farmers in many parts of the world believe in and practice one kind of sympathetic magic or another, following such senseless customs as planting their crops in the proper phases of the moon.

The earliest farmers, however, were not just ignorant and backward *pagani*. They possessed a great store of information which they had acquired through their traditions but which they could also verify experimentally; they checked their data regularly—in fact, they checked them every time they grew a crop. Today we are only beginning to realize how extensive their knowledge was, and how well equipped they were for producing food. Practically all our farm animals and crop plants were domesticated before the dawn of history (coffee is a marked exception). They were not merely domesticated; they were also improved enormously by selective breeding. The early agriculturists took full advantage of the

many useful mutations that appeared, and they altered both their crops and their herds to suit their needs and fancies. "Nature" was changed in many ways, and we still profit from these prehistoric improvements. Many of our domestic plants were modified so drastically, in fact, that we still cannot identify their wild ancestors with certainty.

But long before our progenitors devised a system of agriculture, they had acquired a great deal of biological information. During the ages when they got the greater part of their food from hunting and fishing, they had to have at least a working knowledge of zoology. Hunters naturally have to know the habits of the hunted, and fishermen must be familiar with the behavior of fish. The hunters and the fishermen also had to design their specialized equipment. They had to make it, moreover, out of what we would consider very raw and unpromising materials. The first genius who, starting from scratch, succeeded in catching a fish on a baited hook that he had carved out of a bone, completed thereby a beautiful and complex bit of biological research—worth at least a Ph.D. Perhaps it was even worth the mesolithic equivalent of a Nobel prize.

Before anyone learned how to catch fish, fashion stone projectile points, or chip out effective hand axes (that is, during the pre-hunting, food gathering, or scrounging stage) they had to know their local floras thoroughly. By necessity they were both systematic and economic botanists. They had to recognize the plants that were edible and they had to know which parts of the plants were worth eating. Much of their food came from roots and underground stems. To find this hidden food, they had to be able to identify such plants by their shoot anatomies. It was sometimes a matter of life or death for them to be able to identify the plants that were poisonous, or the plants that would keep them alive in times of famine. (We might mention here that many of our contemporary pre-literate peoples have an expert knowledge of their poisonous plants, and can also recognize the second-rate food plants that may keep them alive when their crops fail.)

The formal history of biology, like all formal history, has a fairly definite beginning. But again, like all such history, its earlier stages would have little meaning if they were divorced entirely from their antecedents. In fact, if we begin our account of biology with the earliest historical records we should be starting somewhere in the middle of the story, and should miss what is in some respects the

most interesting part. Fortunately, modern methods of research have uncovered a great deal of reliable information that bears upon our prehistoric period. Great gaps still exist in our knowledge, but we can present, although in a somewhat sketchy outline, the botanical and zoological information that our prehistoric ancestors possessed. As we have indicated earlier, this information was both important and extensive. It had been accumulating for many thousands of years. In the period that immediately preceded recorded history—that is, between the building of the first permanent settlements and the writing of the earliest records—it reached a rather high level. Most of this biological knowledge was concentrated in the field of agriculture, and we shall begin our account with a brief survey of what was known in this field. To do so, we shall have to examine the origin of our cultivated plants and identify the region in which each plant arose. We shall also have to consider the biological knowledge that their cultivation implies.

When we examine the different kinds of plants that form the basis of our present food supply, and identify their place of origin, we are struck rather forcibly by three facts. First, our really important cultivated plants—the crop plants that furnish the bulk of our food—came from limited but widely scattered regions on three of the six continents. The other parts of the earth furnished very little. Vast areas contributed so little that, if the few plants they did give us were lost, we should scarcely miss them. For example, Europe and temperate North America furnished only a few of the plants worth domesticating, while the continent of Australia furnished none.

Second, our crops have been, with a few notable exceptions, dry-land or even xerophilous plants, thriving in neutral or calcareous soils and yielding only small returns when grown in moist or acid soils. Incidentally, this characteristic of our cultivated varieties gives us important clues as to where and in what climates our agriculture originated—it tells us in what surroundings neolithic man found it profitable to plant and harvest crops.

Third, the basis of practically all our diets—the source of most of our calories—is the seed of one grass or another; that is, the seed of such grasses as barley, maize, millet, oats, rice, rye, sorghum, and wheat. This third point deserves more attention perhaps than it has yet received. It tells us that we ourselves are as much dependent upon the grasses as our cattle, horses, and sheep. It also calls

attention to a major problem that faced the first agriculturists—a major obstacle they had to surmount. Our species is simply not adapted for eating the seed of any grass in its natural state. We are simply not fitted for chewing the crops that we first learned to cultivate. When the earliest farmers tried to eat the products of their agriculture they wore their teeth right down to the gums. Our teeth are no more fitted for chewing untreated grains than are the teeth of browsing animals for grazing.

The first agriculturists were able to thrive on a diet based on the grains only after they had learned how to save their teeth by grinding the grain, removing the husks, and baking the resulting flour into a bread, or, as an alternate, boiling the grains and making them into a porridge. The mere growing of such crops as the grains, crops that had an enormous advantage in that they would not spoil and would furnish food the year round, was not enough to make agriculture an acceptable source of food. The food had to be fitted to the physical requirements of our species. To do this was not easy. The whole complicated technique of changing the grains into something edible was learned slowly and painfully; certainly, directives for it were not handed down from Olympus. The effective technique was the end result of some clever experimentation and much basic and effective research. By the time recorded history appeared, however, the problem had been solved. The men who made and recorded history based their diet on either a bread or a porridge. When the first historians arrived on the scene, their ancestors had been eating bread or porridge for many generations.

Today, we can state categorically that agriculture originated independently in a number of widely separated regions, but we cannot settle the question as to whether the *idea* of agriculture diffused from one place to another. We do know that, in each of the countries where agriculture arose, indigenous plants were cultivated, and that all of our cultivated varieties had to originate in regions where they were native—where their wild ancestors could maintain themselves without human aid. Thus by noting the ecological requirements of our cultivated plants and the geographical distribution of their wild relatives we can get reliable evidence as to where they came from. Thus ecology and plant geography tell us more than the other sciences do about where agriculture began, but they are not our sole sources of information. We can also learn something from the ancient agricultural myths and even from primitive fertility

rites and magic practices. The early historical records also supply some information of value.

Intensive research into the origin of our cultivated plants dates from late in the nineteenth century. In 1882, Alfonse de Candolle published what is perhaps the most famous work on the subject, and subsequent investigations have added greatly to what he discovered. We know now that agriculture did not originate in the lands where the earliest civilizations arose, because the crops that the first civilized men grew, originated elsewhere. The regions that gave us our domestic plants could not support a population dense enough to allow a civilization to begin.

There was one basic flaw in the early agricultural practices, however, which prevented the maintaining of a permanent agriculture in lands where the rainfall was sufficient for the production of good crops. The first husbandmen routinely exhausted the soil because crops always take mineral nutrients away from any field in which they are grown. And where the rainfall is sufficient, the mineral nutrients do not exist in excess. The first agriculturists followed a practice that, unfortunately, has not yet been abandoned. They farmed for a while and then moved away from their depleted fields to lands that either had never been farmed or that had been "resting" for a time. It is possible that the first farmers were almost as nomadic as the first herdsmen. Even today, there are pre-literate agriculturists who have not learned how to restore fertility to their cultivated fields or how to check erosion or prevent the leaching from the soil of the mineral nutrients that are necessary for plant growth. In equatorial Africa the present custom is to clear the land, crop it for about four years, or until it becomes exhausted, and then move on to land that has been resting for about a generation. During the settlement of the United States by Europeans, the exhaustion of the soil and the abandonment of the worn-out farmland was the all too familiar pattern.

Thus a civilization could not arise just anywhere that crops could be grown. It could develop only where the ecological conditions were such that farming could become a stable and settled occupation, and this could occur only where the fertility of the soil was inexhaustible—where the farmers did not have to be continually moving away from their depleted fields. Incidentally, it is worth noting that all civilizations began in regions where the agricultural yields were tremendous even by modern standards.

It is not a mere coincidence that the three earliest civilizations all arose under the same ecological conditions and in surroundings, moreover, that were most unusual. They all arose on the banks of rivers, which is certainly not an odd event in itself; but the rivers all traversed deserts, and rivers that run through deserts are not at all common. The rainless banks of these desert rivers were ideal for growing crops. Desert soils, we know, are exceptionally fertile and, when supplied with sufficient water, produce luxuriant vegetation because, in deserts, there is not enough rain to leach the mineral nutrients from the soil.

The rivers whose banks supported the first civilizations—the Nile, Tigris, Euphrates, and Indus—all furnished abundant water for irrigation. When land is irrigated properly, its fertility can last indefinitely. In these river valleys, the agriculturists no longer had to be nomads. When they became sophisticated enough to discover the value of irrigation and devised techniques for getting sufficient water onto their fields, they could settle down and raise unprecedented quantities of food. Wherever food is abundant, relatively dense populations develop, and where many people live close together, cities can be built, and the arts that depend upon cities have a chance of flourishing.

It would be futile to attempt to discover exactly where agriculture started. Once the *idea* of agriculture arose it could travel. Also, the valuable seed of the first cultivated plants could be transported easily. We know that seeds were carried by the migrating farmers and that they could and did serve as articles of commerce. In fact the archeological evidence indicates that the first cultivated plants were widely dispersed. Recent botanical investigations by Vavilov and others show that some plants continued to spread after they had migrated—from secondary and even tertiary regions of dispersal. Tracing prehistoric plant migrations thus presents many difficulties, and we should be careful not to make our conclusion more precise than the evidence indicates. However, we do have a considerable amount of reliable information. We do know, for example, that only a few of the cultivated species grew wild where the first civilizations arose, and that the food that the first civilized men ate came from plants that had been domesticated elsewhere.

We can also identify a number of broad regions that produced the ancestors of our more widely grown crops. The most important of these regions consisted of Persia and that portion of Asia Minor

that extends south of the Caucasian Mountains. From this region came most of our rosaceous fruits (apples, pears, quinces, plums, cherries, and almonds—although perhaps our apricots and peaches reached this region from China). From here also came most of our cucurbits, our melons, the small grains (wheats, barley, oats, millet, etc.), the brassicas (the cabbage family), peas, the chick pea, pomegranates, etc. From the surrounding lands extending to and including the rest of Asia Minor on the west and Abyssinia on the south, came some varieties of wheat and barley, and such flavoring vegetables as onions, leeks, and garlic. From the northern portion extending around the Black Sea came rye, while from the Mediterranean region came the olive. The fig probably came from Arabia, and much later, from one side or the other of the Red Sea, came coffee.

A second agriculture arose in the region centering around the northern Malay Peninsula. Here rice was first cultivated and the citrus fruits were developed. The hybrid seedless bananas and plantains (first cultivated, perhaps, for their edible stems) appeared first in this region, and here many kinds of beans were grown. Some species of yams were domesticated here and it was in this region that some primitive scientist first learned how to extract sugar from sugar cane.

A third and very different agriculture arose in the New World, which was in truth a new world in more than one sense. As far as its cultivated plants were concerned it might just as well have been on another planet. Merrill has pointed out that not a single species was cultivated in both the Old and the New Worlds, although sister species from such genera as *Gossypium* (cotton) and *Dioscorea* (the yams) were grown in both the Eastern and Western Hemispheres. We have tended perhaps to underestimate the contributions the American Indians have made to modern agriculture, and the common stereotype of the Indian still depicts him as living by hunting and fishing. Actually, when the Spaniards reached Mexico they discovered an entirely new agriculture—one that compared favorably with that of Spain. The American Indians were truly expert agriculturists and their contributions to our own domestic plants were truly major. Today, about one fourth of the farm acreage in the United States is devoted to growing plants that are native to the Americas, and that were selected and developed by the Indians.

From Peru and adjacent regions we get the white potato, maize,

some of our beans, squash, gourds, sweet potatoes, yams, red peppers, and tobacco. From southern Mexico and Guatemala come pumpkins and squash, avocados, chocolate, and vanilla. This region also served as a secondary center where several varieties of maize were developed and where a small red berry that had reached it from Peru grew into our modern cultivated tomato. From the West Indies and northern South America came many varieties of tropical fruits which few of us in the Temperate Zone have ever tasted—although one of them, the pineapple, is eaten and appreciated everywhere. The seedless compound fruit of the pineapple, together with the remarkable cassava native to Brazil, will be considered in some detail later.

The prehistoric story of cotton is important enough to deserve a special treatment. In the account that follows, the data are taken from the work of Hutchinson (1947). Cotton, one of the most useful of all plants, is now raised both for the fiber that grows as a plant hair on its seed coats, and for the oil that is pressed from its seeds. But here is a remarkable fact: most wild species of cotton have no fibers at all—their seeds are naked—and the only useful product that the original cultivators could get from cotton was oil. Cotton, apparently, was first grown in the Indus River valley, but only for its oil. Like the wild cottons it was a tree cotton, but one that came originally from somewhere in Africa, south of the Sahara.

The first cotton fibers that we know of are found in cloth from some of the older archeological layers of the Indus Valley civilization. The fibers were too short to weave into a cloth, however, and they were used merely as fillers in cloth woven of linen. As time passed, the cotton fibers became longer and longer until finally they could be made into a durable cloth without any linen reinforcements. The plant breeders apparently had done a good job. Late in the story, another mutation appeared: the tree cotton produced an annual cotton. Now an annual cotton can be grown in regions where its ancestral tree cotton would be killed by the winters. Hence this mutation was of major importance. Soon after an annual cotton was developed, cotton cultivation spread to colder climates. Cotton became *the* fiber plant of all southern Asia and a little later of China and Japan. It was ultimately grown as far east as Guam. This annual cotton is the diploid (26-chromosome) Old World cotton.

We also have a New World cotton, a tree cotton that was culti-

vated in Peru as early as 1000 B.C. This is a tetraploid (52-chromosome) cotton. Half of its chromosomes are like those of the wild, diploid, naked-seeded cottons native to America. The other half are like those of the cultivated Old World cotton! Here many questions suggest themselves—but no answers. Obviously Peruvian cotton is a species hybrid. It spread to Mexico long before the time of Columbus and there mutated to the annual that was the ancestor of the upland cotton now cultivated in the United States. Later, the cultivation of Peruvian cotton spread to the northern coast of South America, and from this region to the islands off the coast of Georgia. Again it mutated to form an annual, the famous Sea Island cotton, which has the longest, finest fiber of any, vastly superior to that of the Old World cotton. Egyptian cotton is merely transplanted Sea Island. The whole story of cotton is still not known, but the creation of our finest cotton is clearly an achievement of pre-literate man.

The domestication of animals also occurred in prehistoric times. The first to be domesticated was the dog. Indeed it has been said, and not entirely in jest, that man did not domesticate the dog—the dog domesticated man. Dogs were attracted to the earliest hunters and fishermen, who were an exceptionally messy lot, as their kitchen middens prove. Their camps and other habitats must have been very alluring to hungry carnivores. We know how our garbage cans still attract our dogs, and it is possible that the ancestors of our oldest domestic animal were scavengers and bore the same relationship to our paleolithic ancestors that jackals and hyenas bear to the larger cats. The lions and tigers, however, were harder to "domesticate" than primitive man, and the jackals and hyenas have not yet succeeded in securing a stronger species to raise and protect them.

Dogs were useful not only for hunting and as watchdogs, but also for the indirect storage of food that otherwise would spoil. Unlike other domestic animals, their food was the same as that of their masters, who fed them scraps, refuse, and any excess good meat they had in times of abundance. Their masters "recovered" this food by killing and eating their dogs in times of scarcity. The possession of dogs thus made it possible for our ancestors to level out and extend their always precarious supply of food. Dogs were obviously not as effective a storage device as the electric refrigerator but they were the best that paleolithic man had. As late as the eighteenth century, dogs were eaten routinely by many primitive peoples.

Most of the domestic animals originated in the Old World, and only a few in the New. The domestic turkey came from tropical America, and the llamas and alpacas—two members of the camel family—were domesticated by the Andean Indians. These complete the New World's contribution, except for the guinea pig. All other tame animals came from the Old World.

The presence of animals that can be domesticated does not mean that they will be. For example, the American Indians tamed neither the musk-ox nor the bison. Ducks, geese, swans, and rabbits occur in both hemispheres but only the Old World species were adopted and raised by man. The Asiatic elephant and the extinct elephant that lived north of the Sahara were both domesticated, but the African elephant is still wild. Africa south of the Sahara really furnished little besides the guinea-fowl, although the easily tamed zebra and quagga abounded in the region.

The chief source of our domestic animals is the great Eurasian continent. From this landmass we get our sheep, goats, and swine; our asses and several species of horses; our cattle and buffaloes; and our reindeer and camels. Our domestic birds—pigeons, chickens (from India), pheasants, ducks, geese, and swans—also came from Eurasia. The cat was domesticated, assuming that the cat is actually domesticated, in Egypt, but most of our present-day animals came from Asia.

Thus the domestication of animals—except the dog, which is found everywhere—depends more upon the nature and needs of man than upon the animals themselves. Those early men who realized the value of domestication soon had a number of useful animals at their disposal. Those who did not lost a major opportunity for their advancement and remained relatively backward.

The late prehistoric times thus were obviously a most important period for the development of biology. During this period, agricultural knowledge was systematized, memorized, and passed on orally to succeeding generations. But agriculture is not the whole of biology, even though it does impinge on many biological disciplines. In any attempt to record the growth and evolution of our biological knowledge we shall have to explore other fields of pre-history and look for any odd facts that might have some biological significance. Biology, of course, has never existed isolated from other sciences, or even isolated from popular beliefs, and many of these popular beliefs —at least those that we have outgrown—we can now recognize as

erroneous. No history of biology thus can ignore the history of biological errors—nor the subsequent correction of these errors.

Science has always been contaminated in part by the dominant misconceptions and superstitions of the times. The scientists themselves, being human, cannot emancipate themselves or escape from the environment in which their ideas have matured. It is normal for them to bring into their science attitudes and practices that later generations have to discard. The prehistoric biologists were certainly not exceptions and we should not expect either the first agriculturists or the first physicians—the original medicine men—to achieve even our present standards of intellectual austerity and objectivity. In the past, experimental or empirical science has always been mixed with the practice of magic, and both pre- and posthistoric biology contained much that seems to us now to have been very odd indeed. Even the best of the early agriculturists sought assistance from the ancient fertility cults that were widespread and very ancient when history started. Obscene biological practices with magical overtones existed within even the most refined cultures and were designed, apparently, to influence nature (or nature's god or gods) and thus increase the efficacy of the traditional agricultural or medical practices. But the ceremonies and rituals did little practical damage. Biology itself remained healthy inasmuch as it had sources of verifiable information. It was a growing science and thus we know it had a reliable method for increasing the store of existing knowledge.

Prehistoric biology, though badly infected with magical practices and superstition, was both healthy and progressive; but perhaps we shall never learn just how much biology our neolithic and bronze age predecessors knew. We can only infer what they knew from what they accomplished, but we do not even know all that they accomplished. We can get some estimates of the extent of their knowledge, however, when we observe the alterations and improvements they made in the earliest cultivated plants, and when we examine the more significant agricultural practices that were routine when history started.

Long before our ancestors learned to plant and harvest crops, they had an excellent knowledge of the food and drug potentials of their local floras. Obviously, they acquired this knowledge somewhat painfully. They were omnivorous by necessity and they were often hungry. No part of a plant that could be eaten remained uneaten.

If those who feasted on a particular plant died, a new poison was discovered; if they had hallucinations or became intoxicated, a new drug plant was found. Even today, many of the great drug companies send explorers among our pre-literate confreres in search of new drugs. But poisonous or drug plants are rare compared with the total number of food plants or plants that can be eaten in an emergency. The number of different species that furnish food to the primitive peoples of the world is truly astounding. The American Indians, for example, cultivated only about 100 different plants but, as reported by Yanovsky (1936), they obtained food from a total of 1,112 different species belonging to 444 genera. They made tea or salad from the leaves; chewed the stems; dug up the roots, tubers, rhizomes, and corms; ground or roasted the seeds or made them into a paste or a porridge. Where a sugary juice occurred, they extracted it and made it into a wine. They grew tobacco, aged it, and smoked it; they cured their malaria by an extract from the bark of the cinchona tree. They used the dried leaves of the coca (*Erythroxylon coca*) as a narcotic; they learned to extract caffeine from the devildoer (*Paullinia capana*) and use it to relieve fatigue; and they learned to use plant poisons for their arrows, and to catch fish by scattering poison on the surface of the smaller streams and ponds.

In the Eastern Hemisphere, agriculture developed a high degree of sophistication. For example, the art of vegetative propagation —i.e., propagation by cutting, grafting, or budding—is older than history. The olive and other fruit trees were routinely reproduced by these methods wherever they were grown. The fact that superior varieties do not breed true from seed but that a grafted branch bears fruit exactly like the tree from which it came was recorded in the earliest works on agriculture. This fact was recorded, moreover, as something generally known—as ancient knowledge—although the biological principles involved in vegetative reproduction were naturally not understood. Graftings may have originally been an attempt at sympathetic magic—an attempt that just happened to work. At first, it may have been no more than a fertility rite—one that included all the proper sexual symbolism.* This is not re-

* This is indicated by an item that has reached us over a rather devious trail. The following passage is from *The New Golden Bough* (p. 89) abridged from the monumental work of Sir James Frazer, who quoted it from the *Guide to the Perplexed* by Moses Maimonides. *The Agriculture of the Nabataeans,* from which Maimonides quoted it originally, is undoubtedly a tenth-century forgery by Ibn Wahshiya, but in

markable, because the reason for the different yield of vegetatively and sexually reproduced plants was not understood until after the discovery of Mendelian segregation. Our superior varieties of fruit trees are practically all hybrids, and hybrids in general do not breed true.

The prehistoric agriculturists hybridized both their animals and their plants, although the plant hybrids were almost certainly the results of accidents. The production of mules, however, was deliberate and routine. They are mentioned in the earliest records—in Genesis 36:24 and in the *Odyssey* (Bk. XXI)—but in the *Iliad* (Bk. I) true mules were confused with a species of horse, *Equus onager.* Hesiod, in his great agricultural book *Works and Days*, mentions mules four times. In Europe, the cattle also seem to have been hybridized because the evidence indicates that our present breeds are descended from a cross of *Bos primigenius* with *B. longifrons.* The domestic dog crossed frequently with wolves but this need not always have been by design.

Recent genetic research has shown that many of the earliest plants also had a hybrid origin, even though the agriculturists who created them were ignorant of the role of pollen and of the fact that plants reproduce sexually. It is highly probable that they merely followed the common sense practice of selecting seed from their better plants—from plants that united in themselves different desirable qualities—but even so, some of their achievements are startling. For example, the species of wheat that belong to the Spelt group (*Triticum vulgare*) have been cultivated for some 10,000 years. These wheats have 42 chromosomes, but only 14 of the chromosomes are true *Triticum* chromosomes; 14 came from *Agropyron* and 14 from the related genus *Aegilops.*

Hybrid plants have been selected for cultivation in all the great agricultural centers. From southeastern Asia came the seedless triploid banana, a cross between some diploid and tetraploid species. The commercial banana is propagated vegetatively from suckers, cut from the base of the trunk; but to obtain new experimental stock, we must go back to the fertile wild species. The seedless pine-

spite of its spurious nature it does record many ancient agricultural practices: "The work known as *The Agriculture of the Nabataeans* contains apparently a direction that the grafting of a tree upon another tree of a different sort should be done by a damsel, who at the very moment of inserting the graft in the bough, should herself be subjected to treatment, which can only be regarded as a direct copy of the operation she was performing on the tree."

apple originated in northern South America and, of course, from an ancestor that had seeds. From Peru or Guatemala (probably both) came the most puzzling of all cultivated grasses—Indian corn (*Zea mays*). This grain deserves a special treatment in any history of biology.

Indian corn can be described most accurately as a parasite upon the human species. If we did not plant it, it would become extinct. On the other hand, the small grains that were domesticated in the Old World can shatter out of their ears and seed themselves after a fashion. Indian corn, however, cannot. Its seeds are wrapped up in a bundle of husks, and even if the stalk should fall over and the ear be covered with dirt, any crowded seedlings that might sprout will not mature and produce seed for the following year. Obviously the wild ancestors of Indian corn had to be able to reproduce themselves without any extraneous aid. What the wild ancestor or ancestors were like and how the Indian changed them into this most remarkable of grains is a fascinating problem—one that is still the subject of some very fundamental research.

Indian corn forms fertile hybrids with two other American genera —*Euchlaena* (teosinte) and *Tripsacum*. Modern geneticists have even made tri-generic hybrids. Recent studies of chromosome morphology—particularly the studies of the knobs on prophase chromosomes—show that the chromosomes of corn and teosinte have certain knobs in common. As far as we know, either corn or teosinte may have given these chromosomes to the other, although there is a possibility that the chromosomes may have come from a common ancestor. These genera are obviously related, and some investigators have even suggested that the two genera be combined into a single genus.

We now have conclusive evidence that our modern commercial varieties of corn are descended from a number of primitive crosses —such as those between a pop-corn and a pod corn. (Anderson, Beadle, Mangelsdorf, Weatherwax.) We also know that Indian corn had been extensively hybridized long before the Europeans reached the New World, but how much of this hybridization was intentional we have no way of knowing. We have evidence that the Indians were aware of the tendency of different races of corn to mix spontaneously. A number of tribes knew that if they were to succeed in keeping their ceremonial corn pure, they had to grow it in fields well removed from all other corn.

That corn, after it was once domesticated, was subjected to both an intensive natural and artificial selection is clear, because evolution within the species was exceptionally rapid. No one has yet recorded all of the races and varieties the Indians developed. Indian standards of selection were often aesthetic, as is shown by the many different color varieties they created and prized. Each tribe grew its own individual varieties, which, of course, had also to be adapted to the local ecological conditions. These varieties range in size from gigantic tropical types, twenty feet tall and requiring eleven months to mature, to the small four-foot plants that mature in sixty days in the St. Lawrence River valley. Perhaps the most extreme ecological specialization is found in Hopi corn, which grows under xerophytic —even desert—conditions. This dwarf variety germinates and reaches the surface of the ground even when planted from eighteen inches to two feet deep. The Indians learned that this deep planting enabled the young roots to reach the scanty ground water in time to secure enough moisture to germinate and yield a crop worth harvesting even in years of little rain. Other varieties of corn, when planted this deep, die before they reach the surface.

It might be well to emphasize once more that agriculture in the Old and New Worlds originated independently (Curwen and Hatt, Anderson, Merrill). It was not only the cultivated species that were different in the two worlds; the methods of cultivation also were different. Old World agriculture was based upon the plow—surely one of the greatest of all inventions—while New World agriculture depended upon the hoe. The absence of the plow in the New World was not due merely to the lack of animals suitable for pulling the plow. The domesticated New World plants were such that they could be cultivated very effectively with hoes, e.g., corn grown in hills, potatoes in clumps, tobacco set out by hand, etc. On the other hand, the small grains of the Old World could be planted most economically when the seed was broadcast over plowed ground.

One more example of prehistorical research, based on an American plant, will be cited because it illustrates so well both the cleverness and determination of the Indians and the impelling stimulus to research that pre-literate man often had. The research in question enabled the Indians to extract the poison from cassava (*Manihot esculenta*) and use the purified cassava as a major source of food.

The roots of cassava are very nutritious but they contain lethal quantities of hydrocyanic acid. In these roots the food stored by the plant is well preserved from insect pests. It is an ideal source of

food for human beings once the poison is removed. Doubtless, many hungry Indians died of hydrocyanic acid poisoning before they started their research on the problem of eating cassava and remaining alive. Probably many also died during the course of what must have been numerous investigations. The Indians had no simple or easy method of extracting the poison. They had to remove it in a number of steps, taking full advantage of its volatility and of its solubility in water. First the root had to be washed and pared, thus removing the concentration of poison that lies just beneath the skin. The root next had to be grated into a soggy mass which was placed in a *metapee*—a basketwork cylinder that decreased in diameter as its ends were pulled apart. The *metapee* was then stretched between two beams, thus squeezing out the poisonous juice. The grated particles were next sifted through a screen and the remaining hydrocyanic acid removed by heating. The end result was an excellent food. We call it tapioca and we eat it in puddings.

No sketch of prehistoric biology would be complete if we omitted a discovery that resulted from the cultivation of the date palm (*Phoenix dactylifera*). The date palm may even be responsible for the emergence of the earliest civilization for, alone among plants, it could support a fairly dense population even before it was planted and grown in orchards. In those few and limited areas where date forests cover the land, they produce an enormous quantity of food. Dates, moreover, are pleasant to eat, require no special preparation, and furnish, in themselves, an almost complete diet. (Man *can* live on dates alone.) Dates also have a property that makes them invaluable for primitive man. They do not spoil, and can be kept indefinitely. Since they last the year through, there need be no period of scarcity between crops. Any tribe or group who discovered a date forest could enter it and live with little or no labor. Here, fed by the fruits of the earth, and in a climate where clothing was superfluous, they might dwell—for a time at least—in an earthly paradise.

Being human, however, and having an unlimited supply of food, their numbers would increase until some crucial population density was reached—the old Malthusian dilemma—that inevitably would destroy their utopia and force them to seek other sources of food. ("And thou shalt eat the herb of the field and in the sweat of thy face shalt thou eat bread, till thou return unto the ground.") But, as we have stated earlier, a population has to reach a certain density before cities and the arts of cities can flourish. Thus in the regions where date palms grow in forests, we have an ecology where man-

kind might well have advanced from paradise to civilization. Perhaps it was in and around the date forests at the head of the Persian Gulf that mankind first built and dwelt in cities.

The optimum environment for the date palm is most unusual. First, it is a desert plant that thrives in and even requires great heat —up to 130° F. Second, and paradoxically for a desert plant, it needs great quantities of water; but the water need not be fresh. In fact, the date palm does best in water that is slightly brackish. Thus it is ideally suited to oases, where its roots can reach the ground water of the desert. Oases, however, are generally small. The date palm can grow in great forests only in low-lying land that is either swampy or naturally irrigated and is located in a region of great heat and little rainfall. Such ecological conditions occur but rarely. One exists at the head of the Persian Gulf. Here, in the times of the Sumerians, the waters of the Tigris and Euphrates rivers, backed up by the tides of the Persian Gulf, flooded the lowlands with brackish water twice a day, and here forests of date palms covered the land.

The date forests that grew along the lower Tigris and Euphrates rivers could have supported a population that was truly dense by prehistoric standards. Indeed we have indirect evidence that such a population existed there even before the Sumerians reached the region. It was here, where the date palm was first grown in orchards, that a remarkable biological discovery was made—that at least one plant reproduced sexually and that the pollen from the male date palms was the fertilizing element. Of course, hand pollination of the date palm soon became a routine practice, and in time the role of the pollen was simply taken for granted. The practice must have been extremely ancient, for its existence is implied —even if not explicitly mentioned—in the earliest written records.

The discovery of the sexuality of the date palm had such practical consequences and such theoretical implications that we can hardly believe the event would not have been recorded had it occurred in post-historic times. We know that it always impressed the scholars and scientists who traveled into the date palm country and saw it there for the first time. Herodotos described the hand pollination of the date palm in the fifth century B.C., but he confused it with the caprification of the fig tree. A hundred years later however, Theophrastos described it correctly. Formalized representations of the hand pollination of dates are preserved in Assyrian sculpture and on Assyrian and Babylonian cylinder seals. Uncertainties in transla-

tion make earlier records ambiguous, but a business contract of the Hammurabi period (*ca.* 1800 B.C.) mentions the male flower of the date as an article of commerce and, in one translation, the code of Hammurabi mentions pollination. Descriptions of the harvesting of dates go back to the time of Queen Shubad (*ca.* 3500 B.C.).

The discovery of the sexuality of the date palm was one of the greatest discoveries in all agriculture. It resulted in doubling the yield per acre. In a state of nature, half of the trees are males and, of course, male trees do not have dates; but this proportion of male to female trees is necessary if the females must rely on wind pollination. With hand pollination, however, one male tree can be made to serve one hundred females. As the two sexes can be propagated vegetatively by transplanting the suckers that grow from the base of the trunk, the sexes can be cultivated in any desired proportion, because female trees bear female suckers and male trees male suckers. It became easy for the Sumerians to plant orchards with up to 99 per cent of the trees females. Increasing the date-bearing trees from 50 per cent to 99 per cent represented a major advance in crop improvement—greater proportionally even than our twentieth century development of hybrid corn.

Obviously we cannot identify all the biological discoveries that were made during the ten thousand years that immediately preceded history, but in such fields of applied biology as agriculture and medicine we know that the discoveries were numerous and important. Perhaps most of the biological knowledge that was taken for granted in our earliest records was ancient when history started. Of course this knowledge was diluted with many errors, and the prehistoric biologists relied on a number of meaningless formulas. What they knew was primarily empirical—much of it resting on no theoretical foundation whatever—but it was knowledge nevertheless. In agriculture, it was organized and even codified. It was taught and passed on from generation to generation. That it was contaminated with many silly superstitions was to be expected, but this does not invalidate its scientific status. Biology has always included beliefs and doctrines that later generations have had to discard. It would be a bold biologist even today, who would claim that his science is at last free from notions that will have to be abandoned in the future. As long as scientists are human they will bring into their disciplines the beliefs they acquired during their adolescence—beliefs that they cannot support with critical scientific data. But perhaps we are justified in optimistically believing that superstition in the sciences

is in full retreat and that, as time passes, it will infect fewer and fewer fields.

From a practical viewpoint, many of the ancient superstitions were harmless. Farmers who insist on planting their crops during the proper phases of the moon may still be good farmers. Their crops will grow. Even our most scientific calendar, the Gregorian, does not tell us just when to plant our crops. It is really no better than the helical rising of Sirius. In all regions where the seasons vary greatly from year to year, any solar or lunar calendar will be but a clumsy guide to the best times for planting. Self-correcting biological calendars—such as that used by the pre-literate North American Indian—are vastly superior. They planted their corn "when the oak leaves were the size of a squirrel's ear," and thus their calendar was corrected automatically for seasonal variations. These Indians also knew nothing of the potash or phosphate requirements of plants; but neither did our ancestors a hundred and fifty years ago. The Indians really did not need this knowledge, however, because they discovered that if a fish were buried in each corn hill, the crop would be improved.

In the medical sciences, superstition was even more firmly imbedded than in agriculture; and, in medicine, it lasted longer. It is rampant even today in our folk medicine. But sound and valuable practices have existed since the earliest times. Broken bones were set, skulls trepanned, and effective simples prescribed in prehistoric times in both the Old and New Worlds. Any number of instances may be cited. The Andean Indians, for example, discovered the medicinal properties of quinine when they used an extract from the bark of the cinchona tree as a specific for malaria, and they were also well supplied with emetics and purgatives.

Our prehistoric predecessors accumulated a great mass of biological knowledge, which they systematized and used. Their understanding of basic theory, however, was very defective. This should not surprise us, when we consider how very recent it was that we established our own biological theories on foundations we consider to be truly scientific. Much of the historical development of biology consists of improving the theoretical basis on which we organize our growing fund of information, and in rejecting the accumulated errors of observations and interpretation that always accompany a living, growing science.

2

Ex Oriente Lux

The Assyrian cherub who in the eighth century B.C. pollinated the date palm by hand (Frontispiece), possessed a certain amount of biological knowledge. In performing this act, however, he was only following in the footsteps of his Babylonian predecessors who, as we have stated in Chapter 1, had learned that the pollen of the male palm was essential for the setting of fruit. Judging by all the available evidence, however, his knowledge must have remained relatively superficial. The Assyrians almost certainly lacked a real understanding of the principles underlying pollination, because they never seemed to have grasped the idea that pollen was necessary for fertilizing other plants, nor had they learned that all the flowering plants reproduced sexually. What knowledge of biology the Babylonians and Assyrians possessed remained essentially empirical, although it was based on sound observation. Empiricism, as we know, is one of the most important factors in all scientific research; but it is a factor which does not by itself transform knowledge into science.

We would naturally like to know more than we do about the biological knowledge of the first civilized peoples. The evidence that has reached us from those remote times is fragmentary and limited; yet defective as it is, it does give us a relatively well-defined picture. For example, we know that the biology of the Babylonians and the Assyrians was essentially an applied biology, limited primarily to agriculture.

What we know of early Assyrian and Babylonian biology comes

partly from the bas-reliefs they made of their cultivated plants, and partly from carvings illustrating their veterinary medicine. We also find some information in the stone engravings of Hammurabi's *Tables of the Law* (1800 B.C.), where even the remuneration of veterinary surgeons is fixed. We also have a useful treatise in Arabic called "Nabataean Agriculture." Although this is not what it claims to be—the manuscript dates from the tenth century after Christ—it does describe many ancient practices. It is often impossible, of course, to identify the agricultural plants referred to in this document. It has been possible to do so in a few instances, however, thus giving us some knowledge of the way the cultivated plants in ancient Babylonia were used.

Another source for unearthing early biological knowledge is medicine, an art that can be traced in part to the Egyptians, in part to old Indian legends. In Egypt, physicians appear to have had a considerable command of their subject. Ancient medical expositions have been found in several papyri. The so-called "Papyrus Smith," discovered by the Arabs in 1861 and said by experts to date from about 1600 B.C., contained anatomical definitions and descriptions; the Papyrus Ebers, dated 1500 B.C., showed that the importance of the heart was recognized. It also contains scattered descriptions of zoological subjects such as the development of tadpoles into frogs. The remaining portions of these papyri, however, are of a purely medical character, a conglomeration of observations and experiences, mixed with a great deal of magic. It may be assumed that these papyri summarize the accumulation of centuries of medical knowledge, so that they may not represent the first attempts at compiling medical textbooks.

Many centuries before Christ, and in another part of the world, another branch of medicine—pharmacy—had reached an advanced stage of development. The oldest Chinese literature contains the work *Pen-ts'ao*, ascribed to the legendary Emperor Shen-nong (3600 B.C.). This work described the healing powers of plants. It was a *materia medica*, but one that was based on the doctrine of signatures—i.e., red medicaments were especially suitable for the heart, yellow for the stomach, white for the lungs, and black for the kidneys. Diseases of the upper part of the body were cured by the upper parts of the plant (leaves, fruits), diseases of the lower parts of the body by those parts of the plant that grew below the soil, e.g., roots and bulbs. The earliest version of the *Pen-ts'ao* dates from the

Han dynasty (206 B.C.–220 A.D.) although the information it contains is much older. However, Egyptian pharmacy must have had somewhat higher standards than the Chinese because their admirable technique of embalming required an extensive knowledge of the pharmaceutical and preservative properties of herbs.

In addition to the papyri, Egyptian sculptures and other objects found in tombs and pyramids tell us about the medicinal practices of their times. Numerous necklaces have been discovered made of papyrus fibers from which dried parts of pharmaceutical plants hang as pendants, e.g., leaves of wild celery (*Apium graveolens*) and of the blue water-lily, roots of coriander, of cumin, Scilla bulbs and the seeds of Ricinus. A bas-relief from the chamber of Thutmose III in the temple at Karnak (1447 B.C.) shows a picture of a botanic garden (Figure 1), and a traveling dispensary of Queen Mentuhotep

Figure 1. Botanical garden. (Bas-relief in the temple at Karnak, 1447 B.C.)

(11th dynasty, 2160–2000 B.C.) proves that the knowledge of herbs among the ancient Egyptians was far from primitive.

The old East-Indian legends relate that Brahma himself had written a medical work in verse which, as *Ayurveda* ("The Science of Life"), was handed to the gods who succeeded him. When the plague reigned on earth, the god Indra told the god of healing,

Dhavantari, to descend to earth, and there Prince Susrota, who was regarded as his pupil, committed this work to writing. This *Ayurveda* of Susruta is one of the few ancient Hindu manuscripts dealing with medicine which remains extant in the original text as well as in a Latin translation. It is not possible to determine the date of this manuscript with any certainty. Some experts place it in the sixth century B.C., the lifetime of Susruta himself, others merely assume the latest possible date and place it in the eighth century after Christ. But, whatever the age of the manuscript, it may safely be regarded as a record of the medical practices of ancient India.

Because of the mystical inclinations of the Brahmans, their practices are motivated almost certainly by natural-philosophical considerations—i.e., the invisible Brahma, the sourceless fountainhead of all that is created, created the universal ruling principle, *Mahan*, and out of this proceeds Ahankara, who is the one who creates himself and who has the power of observation, of alteration, and of recreation. The five attributes of the elements that Ahankara created are sound, touch, shape, taste, and smell. Brahma, Mahan, and Ahankara, plus these five elemental attributes, form the eight cycles of nature; five sense-organs (ear, skin, eye, tongue, and nose) and five active organs (the voice, the digestive system, the reproductive system, the excretory organs, and the organs of locomotion) together with the five elements (ether, air, fire, water, and earth) are the perceptible creations, and they form, with the addition of reason, the sixteen effects of the cycles of nature. These eight cycles of nature and their sixteen effects combine to form the twenty-four principles of nature.

In addition to this mystical basis, the *Ayurveda* also contains a great many facts of a purely biological character, such as descriptions of poisons, healing plants, and medicaments. It lists some 960 medicinal plants, many of them antidotes and aphrodisiacs, and it includes such soporifics as henbane (*Hyoscyamus niger*) and hashish (*Cannabis indica*). The work gives us a clear picture of the mystical inclinations of the Indian physician. It is a medical textbook and deals with such topics as anatomy, physiology, pathology, and obstetrics. But everything in it is interwoven with innumerable sagas, giving it the appearance of supernaturally revealed wisdom. Its mythological contents, perhaps, were responsible for preserving it as a part of the spiritual tradition of the highest Indian caste, the Brahmans—the keepers of the holy writ.

Agriculture among the Babylonians and Assyrians, and medicine and pharmacy among the Egyptians and Brahmans, are two of the three most valuable sources of our knowledge of the biology of early historic times. A third and later source arose through an interest in every thing that is alive—an interest in knowledge for its own sake —divorced in part from any practical application or from any personal advantage the knowledge might give. For this source, and for this attitude, we are indebted to the Greek philosophers. We are much better informed of the knowledge and ideas of the Greeks than we are of those of the Babylonians, Assyrians, Egyptians, or Indians. The ideas of the pre-Grecian civilizations we have had to glean sporadically from sculptures, utensils, papyri, and a few scattered fragments of manuscripts. The Greeks, however, have left us an abundance of writings and have made their influence felt throughout the centuries.

Myths and legends occurred even in Greek philosophy, but in the sixth century B.C., they began to recede, and observation came to the fore along with some adumbrations of an experimental method. The result of these observations and experiments, however, continued to be incorporated into current philosophical systems. But more and more the observations of the Greeks were corrected by their philosophical schooling and by their training in the logical thinking that is essential for drawing reliable conclusions from masses of data. A harmonious balance between the rational and the empirical methods began to come into being.

The social conditions in the Greece of Homer, in the ninth century B.C., had created a sharp contrast between the aristocrats and the people; but the people began to develop culturally, although for a time they developed very slowly. They learned to read and write and they tried in every way to improve their status and their living conditions. They emigrated in numbers to neighboring countries and established settlements as far away as Sicily and the north coast of Africa. They settled in great numbers on the Aegean islands and on the coast of Asia Minor.

These colonies became lively centers of international trade and traffic, where, liberated from the ties and traditions of the motherland, newer and freer thoughts arose. The Greek spirit was preserved, but its aim shifted; it began its search for a uniform philosophy of the world, and for laws which govern the phenomena of nature. Observations became more accurate and the interpretations

became more logical. The solutions of the problems became more lucid and critical. To the Greek colonials all natural phenomena seemed to demand an explanation: lightning and thunder as well as the migration of birds—rain and wind as well as human diseases. A scientific attitude in the study of nature slowly gained ground.

In this lively, turbulent, seething sphere of trade and traffic, of work and thought, the small town of Miletos played an important part. Its inhabitants traveled widely and became acquainted with the Mediterranean world. They were exposed to new impressions and to the influence exercised by different races with different customs and different conceptions of life.

The much traveled Thales of Miletos (seventh century B.C.) became the leader of the Ionian School of his native city. He became a protagonist of the inductive method and, as he had a typically encyclopaedic mind, he showed a strong tendency to re-examine all available knowledge. With him, the anthropomorphic and anthropocentric theories gave way to those of the *physis*—to those of nature. He held that water was the source of all creation. His pupils and followers, Anaximander (610–545), Anaximenes (560–480), and Anaxagoras (500–428), continued his work, revising and correcting it but maintaining the tradition of the philosophical school of Miletos. The physis remained the creative element, but its character changed: water was replaced first by fire and later by air. Medicine also began to find its way into this natural-philosophical approach: the physis concept gave rise to that of pneuma, the breath of life—the breath that creates and gives a soul to all that is alive.

Other schools of natural philosophy arose. At Croton, in southern Italy, a famous school was founded, led by Pythagoras (582–500). Here Alcmeon (*ca.* 520 B.C.) dissected animals for the first time and demonstrated how arteries were to be distinguished from veins. He discovered the optic nerve. He gave an explanation of sleep, of the origin of semen, and of sense impressions. He was the first to recognize the brain as the seat of intellectual activity. At Ephesos, a school grew up where Heracleitos (576–480) was the dominant personality. At Agrigentum in Sicily lived Empedocles (485–435), whose work in the field of embryology is worthy of note, and who described in fanciful language how natural selection could explain the existence of adaptation in animals.

The motherland in its turn felt the influence of this enlightened

search for a philosophy of nature and for an understanding of the world as a whole. When Anaxagoras returned to Athens from Miletos, he brought the focal point of Greek philosophy back again to the Greek mainland.

Next appeared the school of Cos, where the greatest of all Greek physicians lived. Thanks to Hippocrates (460–376) Greek medical knowledge became a systematized whole; he also accepted a physis as a creative force, but he understood that there were more kinds of physis than merely air or fire or water. Like his predecessors, he had been educated in a philosophical school and he retained the mystical and mythical outlook of the philosophical colleges, but he did survey the entire field of medical science. He was not one to strengthen the factual basis of science by conducting research, or by making dissections or experiments of his own—although his observations at the sickbed were keen and accurate. On the strength of his observations, for example, he was able to connect consumption with tuberculosis of the bones. He was in fact competent in the whole field of medicine. He recognized dryness, moisture, cold, and heat as the four basic factors in human well-being; but he recognized also that not one of these factors was important in itself—only in connection with the others. He saw that in a healthy person a balance was maintained among the four factors, and that a disturbance of this balance leads to illness.

Hippocrates was neither an anatomist nor a physiologist, as some of his predecessors were, but he was familiar with the human body, and with its structure and its activities both in health and in sickness. He was first and foremost a physician and not especially interested in animals and plants per se. But one thing that distinguished him from his predecessors was that he understood the unity and indissoluble interrelationship of the many problems presented by the human body. Thus, in spite of the limitations imposed by his specialized interests, he was a greater biologist than any Greek philosopher-physician who preceded him.

Hippocrates differed from his predecessors in still another aspect: he realized the importance of the environment. His observations made on those individuals who lived in regions with sharply-contrasted and variable climates, led him to advance the thesis that such climates produced a powerful, almost crude, type of inhabitant, whereas an even, temperate climate was conducive to indolence. He went on to draw the general conclusion that every individual

reacts according to his temperament and to the effect on him of his environment. From this, it seemed to follow that human life was divided into seasons just as the climates were: birth, growth, maturity, and decline.

In the writings ascribed to Hippocrates we find biological hypotheses that have persisted until the twentieth century, for example the doctrine that acquired characters are inherited. This belief is actually prehistoric and doubtless arose through a logical connection of two easily made observations: (1) animals and plants can be and are modified by changes in their environment and (2) children do, on the whole, resemble their parents. The Bronze Age myth of Phaëthon depends for this point upon the inherited effects of such parental modifications. Phaëthon, as we remember, drove the chariot of the sun so close to the lands of the Ethiopians that the inhabitants were burned black, and it was the transmission of this acquired character that gave the present Ethiopians their pigmented skin.

In *Airs, Waters, Places* (§ 14), Hippocrates described the origin of the Longheads:

> I will begin with the Longheads. There is no other race at all with heads like theirs. Originally custom was chiefly responsible for the length of the head, but now custom is reinforced by nature. Those that have the longest heads they consider the noblest, and their custom is as follows. As soon as a child is born they remodel its head with their hands, while it is still soft and the body tender, and force it to increase in length by applying bandages and suitable appliances, which spoil the roundness of the head and increase its length. Custom originally so acted that through force such a nature came into being; but, as time went on the process became natural, so that custom no longer exercised compulsion. For the seed comes from all parts of the body, healthy seed from healthy parts, diseased seed from diseased parts. If, therefore, bald parents for the most part beget bald children, grey-eyed parents grey-eyed children, squinting parents squinting children, and so on with other physical peculiarities, what prevents a long-headed parent having a long-headed child? At the present time long-headedness is less common than it was, for owing to intercourse with other men the custom is less prevalent.

Here, we find Darwin's provisional hypothesis of pangenesis foreshadowed. Hippocrates repeated this simplified version of pangenesis in both *The Sacred Disease* and in *On Generation*.

Yet, considered from the biological standpoint, the limitations of Hippocrates' field of interest are to be regretted. Biology, as we know it today, is by no means limited to the study of human life. It includes the vital phenomena of the amoeba as well as those of human beings. It is a marked defect of Hippocrates that he did not

recognize that the knowledge of all living things could be systematized into a science. In some ways, his standpoint was a reaction —a regression compared with that of his predecessors, since they would not have been philosophers if in addition to their medical work they had not given their attention to the problem of problems: the question of how the cosmos and all that it contained had come into being.

They sought and found the answer in the physis, but in the physis they made a somewhat arbitrary choice of one of the natural elements, such as earth, water, fire, or air. They tried to disentangle their opinions from the myths prevalent at the time, and in so doing they opened up a more naturalistic period. Thales and Anaximander, who were keenly aware of the miraculous life of the sea, sought the origin of all existence in water; Thales hesitantly, his pupil Anaximander deliberately. Thales held that the earth was originally in an entirely liquid state, and that, from the elements, as they gradually dried, man was created. At first, our human ancestor was in the form of a fish, but ultimately he acquired a human shape, just as a butterfly starts life in the form of a caterpillar but grows wings later and flies. Anaximenes saw the material constituent of creation as an original mass of mud—a mixture of earth and water—but the actual creative force he found in the air that was influenced by the heat of the sun. The physis thus began to take on another character.

These Greek philosophers all believed in *abiogenesis;* that is, they believed that all living organisms were generated from non-living matter. In later centuries this doctrine was known as "spontaneous generation."

The philosophy of these physicists continued to be tinged with naturalism, although in varying degrees. Heracleitos of Ephesos emphasized a dynamic element, an eternal flux of everything in nature. Empedocles of Agrigentum, who believed firmly in abiogenesis, also conceived of a definite evolution of living creatures. He saw the whole of life as the product of four principles: fire, air, water, and earth. These principles were governed by two ruling forces: the joining constructive force of love and the severing destructive force of hate. First the earth came into existence, then plants, and then, through bud-formation on plants, the animals. All creation was gradual and proceeded through different stages. Dust was formed into the organs of the body—into separate hands, legs

and arms, etc.—and finally, love, conquering all of hate's opposition, combined these organs to form animals. Tentatively at first, there were formed the most widely divergent combinations, the centaurs and chimera of Greek mythology; later, the final groupings of organs produced the animals we know today including, of course, human beings. These latter combinations were fit and adapted to the existing conditions, hence they were able to survive and reproduce; the former combinations were unfavorable, unharmonious, and doomed to a brief and temporary existence. With some indulgence, we can see in this a very primitive adumbration of Darwin's "survival of the fittest." Empedocles explained the process in a somewhat analogous manner: namely, each of the two "seminal liquids" provided specific parts of the embryo; fertilization was the attempt made by these elements to merge, united by love. Cases of abortion were due to an excess or deficiency of the constituents supplied by one or other of the sexes.

Empedocles would not have been a natural philosopher in the true sense of the word if he had not also included plants in his observations. What his theories were on this subject we know only from the comments and quotations that later philosophers such as Nicolas of Damascus and Aristotle have left us in their works. Empedocles' view of plants as living beings might be summarized in three theses, the first being that plants had come into being before the world had completely acquired its present appearance. Their formation occurred in a manner analogous to that of the formation of animals. Here too, the organs—the roots, branches, flowers, and fruits—were formed first out of mixtures of the different elements, the roots mostly from components of the soil, the branches and flowers from air, and the fruits out of whatever was left over. After the separate parts had been formed, they united and formed a unit by the combination of roots, branches, and flowers into the whole plant.

As his second thesis Empedocles stated that plants, like animals, have desires and also sensations of comfort and discomfort, and even a mind and understanding—in short, a primitive form of spiritual life. This view is the logical outcome of his theory that the plant is composed of elements and, since these elements possess an animating factor, the plant also must possess this factor.

The third thesis that Empedocles defended was that plants, too,

possess both sexes, but in plants, the sexes are intermingled and fused and thus each plant by itself can bring forth young.

Trees, which have trunks, notably the olive tree, lay eggs. For the egg is the fruit of the body and from a part of it the animal is formed, the remaining part being nourishment. And from a part of the seed the plant is formed, the remainder being nourishment for the germ and for the first root. (Fragment 79)

Although he speculated freely and was full of imagination, Empedocles was logical and consistent in his deductions. He gave first place to experience and observation, holding that man derives his understanding from his surroundings. He was the last representative of a naturalistic school; his contemporary Anaxagoras turned in a new direction.

The questions that Anaxagoras tried to answer were not restricted to the origin and nature of existence; they included also the essential query, "to what end?" Anaxagoras thus became in some respects the pioneer of a teleological view of life. That is, he attributed adaptations in nature to a deliberate plan. He ascribed the origin of plants and animals to germs present in the air and the ether. According to his opinions, the universe had always existed; everything had always been present in some form or other. The germs of plants and animals were mingled with minerals in one mass of matter; but, guided by the spirit of reason, they separated from it and grouped themselves together. The original *chaos* was both hot and cold, with misty air and ether separated from one another. Out of the chaos were formed water, minerals, and soil, while plant germs floated about in the air. These germs were brought down to earth by the rains and developed into vegetation. Animal germs, however, were confined to the warm ether; they landed on earth in the warm masses of mud, where they were able to develop according to their different properties. The ultimate fate of all animate things therefore was predetermined in the germ. Abiogenesis, he held, did not occur, and the environment was of relatively secondary importance.

Anaxagoras added a second fundamental variant to the biological conceptions of his contemporaries, with the statement that the female organism plays only a passive role in the process of generation and is incapable of making any specific material contribution to the offspring. The male sperm furnishes the only material for the gener-

ation of the embryo, the sperm from the right testis forming a male, that from the left testis a female. He also believed that the material that was to form the embryo was already completely differentiated in the semen. He did not state just how the material got into the semen originally, but he may have had some vague idea of pangenesis. The following quotation is taken from an incidental remark in the Scholia of St. Gregory of Constantinople:

> But Anaxagoras having found an old belief that nothing comes from that which is nowhere, did away with creation and introduced separation in place of creation. For he nonsensically said that all things were blended one with another but were separated out as they developed. For he said that in the same semen there was contained both hair and nails and veins and arteries and nerves and bones and that they happened to be invisible because of their smallness, but as they developed little by little they were separated out. For how could hair be produced from non-hair and flesh from non-flesh?

An important point in which Anaxagoras differs from his predecessors and contemporaries is in his view that plants and animals do differ essentially. It is true he held that there are many important differences between them, but it is also possible to list many points of similarity. Foremost among these comes respiration, which, according to Anaxagoras, occurs in plants as well as in animals.

Our knowledge of the content and extent of the earlier Greek biology comes chiefly from the writings of such philosophers as we have quoted, but the philosophers are not our only source of information. They are merely the source that has been most carefully explored. The Greek poets and dramatists also included in their poems and plays samples of the current biological beliefs, and an intensive survey of their works should give us data that we should otherwise miss. Here we will cite but two of these biological fragments.

The lyric poet Theognis, who lived in the sixth century B.C., accused his fellow citizens of caring more about the breeding of their domestic animals than they did about their own breeding. Obviously the importance of the hereditary factor in shaping human and animal characteristics was known.

> We seek well-bred rams and sheep and horses and one wishes to breed from those. Yet a good man is willing to marry an evil wife, if she bring him wealth; nor does a woman refuse to marry an evil husband, who is rich. For men reverence money, and the good marry the evil, and the evil the good. Wealth has confounded the race. [Cited by A. G. Roper, 1913.]

The second example is from Euripides' tragedy *Electra.* A servant finding a lock of hair left by Orestes on his father's grave, attempts to identify Orestes by its resemblance to his sister's.

> But view these locks, compare them with thine own
> Whether like thine the color; Nature loves,
> In those, who, from one father draw their blood,
> In many points a likeness to preserve.

Obviously, family resemblance in hair types was known.

Each of the Grecian philosophers was a distinct personality, with highly individualistic views. They built on the knowledge of their predecessors, but each one revised and developed his own ideas according to his own experience and understanding. Not one of them, however, left a stamp on science as lasting as that of Plato's pupil Aristotle (384–322 B.C.).

When Aristotle was twenty years old, he met Plato and, until the latter's death seventeen years later, he remained his student though by no means his spiritual follower. In spite of the overpowering influence of Plato, who was forty years his senior, Aristotle remained an independent thinker. Shortly after the death of Plato (346 B.C.) Aristotle left Athens to settle in Asia Minor, and it was here in 343 B.C. that Philip of Macedonia summoned him to his court to take charge of the education of Philip's son Alexander, later to be called "the Great." This new appointment enabled Aristotle to develop his talents and display them to the full. The broad-minded Alexander always looked upon him as a personal friend (but not quite as an *almus pater*) and assisted him throughout his own short life. Apparently, both Alexander and Aristotle felt themselves to be world conquerors and reformers, Alexander with his military genius, Aristotle with his scientific gifts. Each recognized the value of his own personality; but, in addition, each recognized the other's great qualities. Aristotle was convinced that he had something new to give to the world and stated with modest assurance that he did not find the way prepared for him, nor any examples that he might follow. He merely took the first step in a new direction, and therefore it was a small step—although he had given much thought and much work to it. His work must be regarded as a first attempt and thus judged with indulgence. And indeed he well deserves our sincere appreciation, although we may be critical of a number of his theories.

Aristotle's four best-known biological works are *On the Soul, The History of Animals, The Parts of Animals,* and *The Generation of*

Animals. He was broad-minded and critical in his approach, and held that we should not, with a certain petty fastidiousness, brush aside the study of animals less important than ourselves, since there was something of the marvelous in all natural things because not a single one of them lacked nature and beauty. He was also of the opinion that we should not deduce a general rule merely from a process of logical reasoning; but that we must prove that it could be applied to each fact. For it was in the facts that we must seek the general rules, which must always be in agreement with the facts. Experience furnishes us with definite data and the path from the data to general rules was induction.

Aristotle saw natural science as a whole made up of many divisions, to which we would now give such names as botany, zoology, embryology, anatomy, teratology, and physiology. But above all he saw that, however different and unconnected these divisions may be, they form one indivisible whole, *biology*. He accepted all kinds of transitions from inanimate substance to plants, from plants to the plant-animals (animals attached to the bottom of the sea), and thence to the other animals. Every living organism, however, he believed to be endowed with a soul, which manifested itself in its organization, and every organism possessed, in its highest form, the faculty of knowledge, feeling, thought, desire, will, self-movement, growth, and the attainment of maturity. In plants, this soul was limited to the nourishment imbibed through the roots. Animals not only nourished themselves, but were conscious also of what happened around them. The fact that they are warmer than plants was linked to their will and to their power of motion.

Man represented the highest stage in the development of the soul; he stood close to the gods, yet he was but a link in the evolution of the organisms, even if he distinguished himself from the animal by his ability to form ideas and, by means of thought, to abstract and to generalize them.

Aristotle was the first to attempt to unite every type of animal into a systematic scheme, one in which he contrasted the animals that had blood with those that were bloodless. The animals with blood (*enaima*) included those grouped as the mammals (except whales), birds, oviparous quadrupeds (reptiles and amphibians), whales, and fish. The bloodless animals (*anaima*) were divided into the Cephalopoda, higher Crustacea, Insecta, and Testacea, the last

group being a collection of all the lower animals. Aristotle was the first to show any understanding of an existence within the taxonomic system of units of different degree. But his concepts, *genos* and *eidos*, cannot be identified with our concepts, genus and species, since they were not absolute categories but only of relative status in comparison with the larger or smaller groups.

Aristotle recognized the value of analogy in the structure of different types of animals, but here he also took into account the animal's mode of life. Thus he classified the ostrich and the bat as belonging between mammals and birds. Whales appeared to him to resemble mammals anatomically but, nevertheless, he classified them with fishes. He saw the body as composed of the primary substances (fire, earth, air, and water), which were organized into both the homogeneous and the heterogeneous parts. This was actually placing the study of the body in three fields, which today we label *organic chemistry*, *histology*, and *anatomy*. He stressed the analogies between bones and fish-bones, nails and claws, the hand and a lobster's claw, and bird feathers and fish scales.

His theories about the internal processes taking place with organisms were primitive and, in our eyes, fantastic and unimpressive. But should we, spoiled and pampered as we are by our tools and instruments, judge these theories harshly?

The most important section of Aristotle's work is undoubtedly that devoted to reproduction and to the kindred subjects of heredity and descent. He distinguished four causes by which organisms could be generated. First, abiogenesis: where a part was separated from a mass of mud or of muddy matter. The breath (*pneuma*) and heat were embedded in this part. From this mass originated some plants, insects, crustaceans, and fishes. Second, bud formation: where small animals were formed on the sides of larger ones, as happened with plants or crustaceans. Third, sexual reproduction without copulation: as in plants, bees, and some fishes where male and female attributes were merged to such an extent that no copulation was needed (a cross between our concepts of hermaphroditism and parthenogenesis). And lastly, fertilization and copulation. This form of reproduction he described in detail: in male organisms he described the sperm, in female organisms the ovum or, in the case of viviparous organisms, the menstrual blood. The fact that he considers the menstrual blood as the actual female generative substance

was connected with his theory that blood was the final product of the process of nutrition and that the blood of the female organism was less perfected than that of the male.

"The female organism supplies the matter, the male the form and the principle of movement." These words summarize Aristotle's viewpoint regarding the process of fertilization. In the word "movement" we must include our modern concepts of differentiation and development. He knew of four forms of movement: (1) movement of substance—that is, the generation and destruction of substance; (2) movement of a quantitative nature—growth or diminution, or we might say the gain or loss of bodily parts; (3) movement of a qualitative nature—alteration of matter, metamorphosis and functional change in organs; (4) locomotion—change of place of organs. This fundamental distinction between the substances issuing from the male and from the female pervades the whole of Aristotle's natural philosophy. In this respect his observations are much more acute and more accurately defined than those of his predecessors.

The principle of "movement" or change can be found everywhere in nature. He believed that nature advanced gradually, from the inanimate substance to the animal, in ways that make it impossible to draw sharp lines of demarcation, or to decide on which side of such a line a certain intermediate form should be placed. This same spirit is reflected in his discussions on heredity and descent. He was confident that nature did nothing without a purpose. She strove always to attain the greatest possible beauty. The antithesis between *preformation*—the predetermination of all the stages through which the organism develops—and *epigenesis*—the influence on this development of exterior forces—which was to play such an important part in later times (the times of Leeuwenhoek and Swammerdam) had already found an expression in the works of Aristotle, and he proved himself a convinced protagonist of the epigenetic conception.

Aristotle believed that a most promiscuous hybridization occurred in nature and that this hybridization resulted in the production of many new species, because he thought that all hybrids were fertile except the mules. (From *The Generation of Animals,* Bk. II, chap. 7):

> Natural intercourse takes place between animals of the same kind. However, those also unite whose nature is near akin and whose form is not very different, if their size is much the same, and if the periods of gestation are equal. In other animals such cases are rare, but they occur with dogs and

foxes and wolves; the Indian dogs also spring from the union of a dog with some wild dog-like animal. A similar thing has been seen to take place in those birds that are amative such as partridges and hens. Among birds of prey, hawks of different form are thought to unite, and the same applies to some other birds. Nothing worth mentioning has been observed in the sea, but the so-called "Rhinobates" is thought to arise through the union of the "Rhine" and "batos." And the proverb about Libya, that "Libya is always producing something new," is said to have originated from animals of different species uniting with one another in that country.

Aristotle did not believe that species were stable and unalterable. He thought that new species came into being as the occasion demanded, both by such hybridizations as we have listed above and by a direct adaptation to environmental changes. He believed explicitly in the inheritance of acquired characters, from *The Generation of Animals*, Bk. I, chap. 17:

And these opinions are plausibly supported by such evidences as that children are born with a likeness to their parents, not only in congenital but also in acquired characteristics; for before now when the parents have had scars, the children have been born with a mark in the form of a scar in the same place, and there was a case at Chalcedon where the father had a brand on his arm and the letter was marked on the child, only confused and not clearly articulated.

But, needless to say, Aristotle was not a pre-Darwinian evolutionist. Organic evolution implies much more than the mere existence of an aggregation of unstable, changing, and fluctuating species.

Actually, Aristotle rejected natural selection. Apparently Empedocles' description of nature's preserving the fit or the well-adapted and discarding the unfit was known well enough to merit a rebuttal. And Aristotle set out to rebut it. He recognized clearly that natural selection could explain the fact of adaptation and that if the selective power of nature were admitted, there would be no need for teleology. But for Aristotle, as for the Aristotelians of today, teleology is sacrosanct; he is really the type specimen of the teleologist. In his *Physics* (Bk. II, chap. 8) he stated:

. . . Why not say, it is asked, that nature acts as Zeus drops the rain, not to make the corn grow, but of necessity (for the rising vapour must needs be condensed into water by the cold, and must then descend, and incidentally, when this happens, the corn grows), just as, when a man loses his corn on the threshing-floor, it did not rain on purpose to destroy the crop, but the result was merely incidental to the raining? So why should it not be the same with natural organs like the teeth? Why should it not be a coincidence that the front teeth come up with an edge, suited to dividing the food, and the back ones flat and good for grinding it, without there being any design in the matter? And so with all other organs that seem to embody a purpose. In

cases where a coincidence brought about such a combination as might have been arranged on purpose, the creatures, it is urged, having been suitably formed by the operation of chance, survived; otherwise they perished, and still perish, as Empedocles says of his "man-faced oxen."

Such and suchlike are the arguments which may be urged in raising this problem; but it is impossible that this should really be the way of it. . . . Accordingly, if the only choice is to assign these occurrences either to coincidence or to purpose, and if in these cases chance coincidence is out of the question, then it must be purpose. But, as our opponents themselves would admit, these occurrences are all natural. There is purpose, then, in what is, and in what happens, in Nature.

One might, as Radl does, see two characteristic principles underlying the natural philosophy of Aristotle: (1) a *dynamism*, with power, energy, and will being predominant, whereas matter remains of subsidiary importance; and (2) a universal *vitalism*, wherein the whole evolution of the world is considered a complex of living phenomena. To him even the movement of an inanimate stone falling is organically linked with cosmic events, just as we conceive the circulation of the blood to be connected with the life of the organism. Biology is the science of sciences; physics, chemistry, and astronomy are only some of its component parts.

The obvious question arises, why did a universal genius like Aristotle pay so little attention to plants? The answer is that he did actually write botanical works, but that his two treatises—*A Theory of Plants*, and one that is thought to have been called simply *Concerning Plants*—were both lost. All we know about these is derived from the works of subsequent writers, whose comments unfortunately are often unreliable. Another source of information about his botanical theories is the work of his pupil Theophrastos of Eresos (372–288 B.C.), to whom Aristotle left his comprehensive library. Theophrastos has left us two botanical works: one about the history of plants and another about the origin of plants. His work is a welcome complement to Aristotle's to the extent that it applies Aristotle's ideas to plants and develops botany along Aristotelian lines, although it gives far less prominence to its philosophical basis and expresses considerably less original thought.

Evidently Theophrastos liked to grow plants, and many of his observations are drawn from his agricultural experiences. He shows himself a very acute observer. In his work he lays down the elementary principles for an anatomy of plants, and he distinguishes between the external parts as organs and the internal parts as tissues. He saw that these, in turn, can differ and take the form of fibers,

veins, flesh, wood, bark, marrow, and sap. He grouped plants into trees, shrubs, and herbs; into fruit-bearing and barren; into flowering (*phanerogams*) and non-flowering (*cryptogams*); and into evergreens and the deciduous. He recognized the various methods of reproduction and multiplication—by means of seeds, tubers, grafting and inoculation. He also observed that plants can move.

The historian of botany, Edward Lee Greene, has abstracted a number of the other observations that Theophrastos recorded. Theophrastos tells us more about plants than all of his predecessors put together, and he well deserves to be called the "father of botany." His knowledge of plant anatomy was fundamental and detailed. He noted, for example, that aerial roots were really roots and were quite distinct from tendrils and that corms and rhizomes were underground stems and not just modified roots. He recognized three types of stems—trunks, stalks, and culms. He held that the calyx and the corolla were formed of metamorphosed leaves and in this he was given credit by neither Linnaeus nor Goethe. He recognized the importance for classification of hypogynous, perigynous, and epigynous flowers. He distinguished between centripetal and centrifugal inflorescence, and also between petaliferous and apetalous flowers. He was the first to use the word "fruit" in the technical sense as the pericarp. He distinguished the characters in leaves and stems that separate the monocotyledons from the dicotyledons. He interpreted correctly the formation of annual rings.

Theophrastos wrote the earliest detailed description that has come down to us of the hand pollination of the date palm. Then he gave us the first unambiguous account of sexual reproduction in the flowering plants. In his *Inquiry into Plants* (Bk. II, chap 8, § 4) he stated:

> With dates it is helpful to being the male to the female; for it is the male which causes the fruit to persist and ripen and this process some call, by analogy, "the use of the wild fruit." The process is performed thus: when the male palm is in flower, they at once cut off the spathe on which the flower is, just as it is, and shake the bloom with the flower and the dust over the fruit of the female, and, if this is done, the female retains the fruit and does not shed it. In the case both of the fig and of the date it appears that the "male" renders aid to the "female,"—for the fruit-bearing tree is called "female"—but while in the latter case there is a union of the two sexes, in the former the result is brought about somewhat differently.

But the date palm was not the only plant that Theophrastos recognized as sexual. In the following passage he showed clearly that

he knew the difference between male and female flowers (from Bk. I, chap. 13, § 4):

> Again some flowers are sterile, as in cucumbers, those which grow at the ends of the shoot, and that is why men pluck them off, for they hinder the growth of the cucumber. And they say that in the citron those flowers which have a kind of distaff growing in the middle are fruitful, but those that have it not are sterile. And we must consider whether it occurs also in any other flowering plants that they produce sterile flowers, whether apart from the fertile flowers or not. For some kinds of vine and pomegranate certainly are unable to mature their fruit, and do not produce anything beyond the flower.

We should emphasize, perhaps, one other aspect of Theophrastos' thought even though its intrinsic merit is not great. He was convinced that all species were unstable and that they changed (we would say evolved) as the occasion demanded. He was, of course, quite wrong; he, like Aristotle, cannot be ranked among the pre-Darwinian evolutionists. Actually, the stability of species had to be firmly established before their gradual and orderly alterations could be identified—the alterations that we call organic evolution. But any history of evolution will have to be concerned with his concept of species.

Theophrastos devoted almost an entire book (*Inquiry into Plants*, Bk. II) to describing how plants changed their species. He stated categorically that plants changed their species when they were transplanted to a different country, and that the change was caused by their growing in a different soil and living in a different climate. He also noted the difference between cultivated plants and their wild relatives, and he assigned this difference to prolonged and skillful cultivation.

He was especially impressed by the fact that many plants did not breed true from seed, and he noted particularly that no cultivated fruit trees ever bred true. The farmers, as well as the philosophers, knew that whenever fruit trees reproduced by seeds, as they did in nature, they routinely produced new varieties. He described how one species of mint changed into another, how wheat changed into darnel, and how one kind of fruit changed spontaneously into another. He even gave specific instructions for changing one-seeded wheat and rice wheat into the more valuable wheat that was in general cultivation. He did not confine himself to the plant kingdom but stated that animals mutated more than plants did because, in their migrations from place to place, they experienced many environmental changes, while plants grew anchored in one spot. He

described, for example, how the water snake changed into a viper when the marshes dried up.

Theophrastos was of a more sober temperament than Aristotle, whose teleological principles he found to be much too pronounced. He held that though Nature does possess all the elements in herself, the agriculturalist still can help her attain her ultimate purpose.

With Aristotle as a general biologist and, especially, as a zoologist, and with the less great but indubitably important botanist, Theophrastos, a period of nature investigation drew to a close—a period enchanting in its love for pure science and strong and healthy in its originality. However naive and childish a modern biologist may think Aristotle's method of approach was, he will nevertheless have to recognize that for acuteness of thought and for a scholarly treatment of the limited but often misleading facts at his disposal, his mind may serve as an example to us all. Aristotle dominated all science for some two thousand years—something that no other scientist can ever do.

3

The Hellenistic-Roman Period

With the conquest of the Persian Empire by one of Aristotle's pupils, Alexander the Great, a new era came into being. The intellectual dominance of Athens came to an end—although Athens would remain an important cultural and scientific center for some centuries to come. Alexander had succeeded in bringing the entire Middle East under the military and cultural hegemony of the Greeks and Macedonians. He had conquered the Persian Empire, the largest the world had ever known, but after his death in 323 B.C. his lieutenants partitioned his empire and, out of the fragments, built a number of new states which were Grecian in composition and culture. An upper class of Greeks ruled the lands where civilization itself had begun, and Greek standards in art, philosophy, and science replaced those of the older nations. This era in history is called the Hellenistic Age. It was an age of military turmoil—of wars, of conquests and of shifting national boundaries—but it was also an age of great cultural advance. Scholars, philosophers, and artists were held in high esteem. They were sought after and supported, and some who became famous were even collected by the more enlightened of Alexander's successors. Never before had an intellectual elite been so cherished and so honored.

New centers of learning arose; new schools and new research institutions were founded. New cities became political and cultural capitals. The more enlightened rulers sought the prestige of having the better known scholars or artists at their court and of supporting some outstanding institution of learning. The most famous of all

such institutions was the Museum at Alexandria. Other temples of the Muses were established in such centers as Antioch and Pergamon. Commerce was thriving and new centers of trading and of culture arose. Smyrna became one of the wealthiest of cities, a potent rival of such older cities as Miletos and Ephesos.

The Hellenistic Age was an age of commercial enterprise, of growing wealth, and of safe and extensive travel. The expanding Greeks fused with and absorbed the better elements of the indigenous population that they had conquered, and were influenced in turn by the older civilizations. All in all the Hellenistic Age was a period of great prosperity. It lasted about three hundred years—from about 300 B.C. to the time of Christ. It started with a great rebirth of energy and intellect and it ended in a slow decline. But before it fell too low, it was fused into the *Kulturkreis* of the vigorous and expanding Romans.

Alexandria *near* Egypt was founded by Alexander himself on the western end of the Nile delta. It was connected by Lake Mareotis to the western branch of the Nile. It became the governmental center of Egypt and, because of the great library in the Museum and also because of the scholars and scientists who lived and worked at the Museum, it became the intellectual center of the world. So great was its prestige that sometimes the whole Hellenistic Age is called the "Alexandrian."

During the first hundred years of its existence, the Museum served as a focal point for the thinking fraction of mankind and, for another two hundred years, it remained one of the great centers of scholarship. It survived the Roman conquest and even prospered after Egypt became a Roman province. Several Roman Emperors sought to restore and preserve its ancient glories. But, with the growth of Christianity, the Museum declined and in the fourth century after Christ (390 A.D.) it was destroyed by Theosophilos, the fanatical Bishop of Alexandria. Any remnants of the library that might have escaped the religious fanatics were obliterated when the Muslims sacked Alexandria in 645 A.D. But for nearly five centuries the Museum at Alexandria was the scientific center of the world.

When tracing the growth and transmission of biological knowledge, it is well for us to remember that there were no professional biologists in the Greco-Roman period. Today, what we call biology was then but a minor segment of philosophy. Even as late as the nineteenth century the natural and physical sciences were often

lumped together as natural philosophy. But the two great fields of applied biology—agriculture and medicine—were well organized and distinct disciplines from the earliest times. Thus, if we are to follow the evolution of biology during the Hellenistic and Roman periods we shall have to consult the writings of the physicians and agriculturists as well as those of the philosophers.

To many philosophers, biology was only an incidental and peripheral subject—it was merely a segment of the factual equipment they needed to follow their calling. Even Aristotle's zoology was only a single facet of his many-sided interests. As we have stated, Aristotle began his studies as a follower of Plato, but his early writings, which record this phase of his development, exist now only in a few scattered fragments. They influenced the thinking of the classical period, however, especially in the Latin West. The works of Aristotle that have been preserved—although a mere fraction of the total—show the wide range of his activities. In addition to his four major treatises on animals, he wrote on logic, on the theory of knowledge, on mathematics, astronomy, mechanics, physics, music, meteorology, chemistry, geography, geology, psychology, medicine, politics, sociology, and ethics.

Theophrastos was also a polyhistor, and we know him primarily as a botanist simply because his two great works on botany have been preserved. We have only fragments of his other writings, but these fragments show us that he wrote also on the winds, on odors, signs of the weather, stones, poisonous animals, dizziness, sweat, and weariness. But his works on arithmetic, geometry, and astronomy are lost.

Nothing like our twentieth-century specialization ever existed in the classical world, and practically everyone whose biological contributions we shall examine had broad and scattered interests. Often, the many different fields in which they labored seemed to have little or nothing in common, but it is from the works of such men that we get our knowledge of the biology of the Hellenistic-Roman period.

The Hellenistic Age *sensu stricto* constitutes only the first third of the whole Hellenistic-Roman era, which lasted from around 300 B.C. to about 600 A.D. Many social, political, and religious changes took place during these nine centuries, changes that had a profound influence on the growth and progress of science and, ultimately, on its degradation. Biology, as a whole, advanced and retreated along

with the other sciences, except that portion of the science that was incorporated in agriculture. This portion of biology seemed to be immune to the influences of the fluctuating philosophies and to the other social and cultural changes. Classical agriculture, while inefficient by our modern standards, seems to have had a built-in buffering system, one that protected it against the deteriorating intellectual standards that foreshadowed the collapse of the Mediterranean civilization. In fact, the history of Hellenistic-Roman agriculture differs so markedly from that of the other biological sciences, that it seems best to record it separately and in another chapter.

The biological advances made in the Hellenistic Age begin appropriately enough with the discoveries made by some of the most famous scientists of antiquity, the physicians who were connected with the Museum at Alexandria. Chief of these was Herophilos of Chalcedon, who was born late in the fourth century B.C. Almost alone he founded anatomy as a distinct discipline. He wrote three books on human anatomy and one on the eyes. He also wrote a textbook for midwives. He is said to have been the first physician to have dissected the human body. As he was the first he naturally made a number of discoveries and he clarified and gave a precise meaning to many of the anatomical terms we still use. He described the brain in detail, he distinguished between the tendons and the nerves, and he interpreted their functions correctly.

He named the duodenum and he described the liver, the salivary glands, the pancreas, and the genital organs. He distinguished between the arteries and the veins and he held that they both contained blood. This would seem to be a very trivial and redundant observation, except for the fact that many of his contemporaries believed that the arteries were filled with air.

This rather common mistake helps to emphasize the difficulties of the early anatomists and physiologists. They did not have an adequate experimental technique and they lacked the fundamental knowledge that would serve as a check to their observations. They knew that air had to go into and out of the lungs and that air was necessary for life. They also recognized the explosive and propulsive forces of the gases that formed in the intestine. A simple analogy caused them to believe that the semen was discharged by a blast of air and that only the semen that was foamy could fertilize. Semen which sank in water was supposedly sterile.

Herophilos used the clepsydra to measure the pulse and he found

that the rate at which the pulse beat was of great value in diagnosing fevers. He introduced many new drugs but often resorted to bloodletting as a part of his healing technique.

A younger contemporary and colleague of Herophilos, Erasistratos of Iulis, was born about 304 B.C. on one of the islands in the Aegean Sea. He also lived in Alexandria and worked at the Museum but, according to Nordenskiöld, he and Herophilos became bitter rivals. He does not seem to have been quite the anatomist that Herophilos was, but he accomplished more in the field of physiology. He undoubtedly dissected living animals, and he was accused of dissecting living human beings. The accusation may well be false, but Erasistratos lived at a time when criminals were sometimes tortured to death, so any human experimentation he may have indulged in could have been relatively humane by the standards of the times.

He tried to explain all biological functions by natural causes and he rejected completely every explanation that was based on the supernatural. He believed that the arteries were filled with air, and this very common error may well have kept him from discovering the circulation of the blood. Even so he came close to making that discovery, for he showed that the arteries and veins were connected with each other through the capillaries. He seems to have been the first physician ever to have distinguished clearly between the principles of hygiene and those of therapeutics. He was opposed to violent cures, to too much bloodletting, and to the excessive medication that so many physicians resorted to.

A contemporary of Herophilos and Erasistratos, Nicander of Colophon (a city on the Aegean Coast near Ephesos), flourished around 275 B.C. We have no evidence that he did any biological research himself but he was interested in many biological fields and we are indebted to his writings that have survived for some of our knowledge of the biology of the third century before Christ. Nicander can be described as a talented amateur. Professionally, he was a priest of Apollo, but his avocation was writing bucolic poetry. He could also be described as a pharmacist and a toxicologist.

He wrote poems on many subjects. He wrote on agriculture, on apiculture, and on snakes. He wrote on prognostics and on cures. His *Theriaca* is a long poem on poisonous animals. It describes the effects of their venoms and it lists the available antidotes. His *Alexi-*

pharmaca is also a long poem, dealing with poisons in general and with their remedies. Nicander lists 125 different plants in these two poems. He was also the first man to record the therapeutic use of leeches.

Except for a few works on agriculture (See Chapter 4), little of biological interest has come down to us from the second century before Christ. In the first century, however, several outstanding philosophers and physicians produced works of real biological importance. The first of these was Cratevas, who was the personal physician of Mithridates IV, King of Pontus. The king himself was a learned man, an herbalist—a rhizotomist—and, as a king, he had a professional interest in poisons and in discovering all possible antidotes. In Pontus and its neighboring kingdoms, poison was a long-standing occupational hazard of kingship and Mithridates was only taking routine precautions.

He was far from routine, however, in his researches, which were based on sound biological principles. He tried to build up an immunity to poisons by administering sub-lethal amounts and then gradually increasing the dosage. His physician, Cratevas, also was very interested in poison and he investigated the actions of metals on the body. We remember Cratevas, however, because he wrote an herbal that was *illustrated*. It is even possible that some of these illustrations were copied in a sixth century manuscript of Dioscorides that is now in Venice. Cratevas also wrote a materia medica.

Cassius Dionysios of Utica flourished around 88 B.C. He wrote on botanical subjects and translated the agricultural work of Mago the Carthaginian from Latin into Greek. He also added some Grecian material to Mago's work which, of course, had first been written in Punic. Dionysios is now credited with having written an illustrated pharmacopoeia.

Unlike his contemporaries Cratevas or Cassius Dionysios, Nicolas of Damascus (b. 64 B.C.) was not a biologist or even a philosopher. His importance in the history of biology is due to a series of accidents. Nicolas was the friend and secretary of Herod the Great, who was King of Judea from 40 to 4 B.C. Herod greatly admired the Hellenistic-Roman culture and he tried to re-orient the Jews away from the Aramaic world and into the world of the Greco-Romans. Apparently, he relied on Nicolas for encouragement, advice, and

assistance. Nicolas could really have been the *eminence gris,* who was responsible in part for the acts of Herod—for the acts of one of the major villains in the Christian calendar of sinners.

In addition to being a royal secretary, Nicolas was a historian. Among his many writings was a history of the world in 144 books, a history that began with the creation and ended at the time of Herod. The interest we have in Nicolas is due not to his historical writings but to what appear to be two chance events: (1) he wrote a short treatise on botany, *De Plantis,* which has survived, and (2) the authorship of his work was ascribed to Aristotle. Indeed, *De Plantis* is still included in the Aristotelian Corpus.

The Greek original of *De Plantis* is lost, but it was translated into Arabic. The Arabic translation is also lost, but it was translated into Latin. In medieval times, the Latin version was translated back again into Greek. *De Plantis* is a real botanical work and not just an ancient herbal. Its quality is best attested by the fact that it was ascribed to Aristotle. It is definitely in the Peripatetic tradition and, next to the contribution of Theophrastos, it is the best work in the field. If the botanical books of Theophrastos had suffered the fate of his other writings, we should have to rely on Nicolas for almost our entire knowledge of a classical botany.

The intellectual climax of this last century before Christ was reached in *De Natura Rerum* of Titus Lucretius Carus (99–55 B.C.). Lucretius was one of the greatest thinkers of the age—or of any other age for that matter. Intellectually, he would have been more at home in the twentieth century than in even the sophisticated Roman circles in which he moved. He was a poet who composed but one poem, but it was the greatest philosophical poem of all time.

Lucretius sought to vindicate the rights of reason and of the intellect and to discredit all superstition and all explanations that were based on the supernatural. Only a small portion of his great poem was devoted to biological subjects, and he had few data on which to build, but he could think clearly and he could draw logical conclusions from the few facts that he had. He had a real facility for guessing correctly, and he was lucky more often than he had a right to be purely on a basis of chance. He was an outspoken anti-teleologist and, in order to explain the adaptations that teleology could account for—explain the fitness of animals and plants to their conditions of life—he adumbrated natural selection.

Lucretius saw very clearly that teleology and natural selection

were rival hypotheses and, as he was a rationalist, he preferred natural selection. Except for the figurative language and background speculations, the following passage in *De Natura Rerum* seems modern. Lucretius recognized that natural selection implied the production of both the fit and the unfit, and he saw clearly that the unfit had to perish. The influence of Empedocles on Lucretius is shown clearly in this quotation (from Bk. V, lines 837–878, tr. by W. H. D. Rouse):

> Many were the monsters also that the earth then tried to make, springing up with wondrous appearance and frame: the hermaphrodite, between man and woman, yet neither, different from both; some without feet, others again bereft of hands; some found dumb also without a mouth, some blind without face; some bound fast with all their limbs adhering to their bodies, so that they could do nothing and go nowhere, could neither avoid mischief nor take what they might need. So with the rest of like monsters and portents that she made, it was all in vain: since nature denied them growth, and they could not attain the desired flower of age nor find food nor join by the ways of Venus. For we see that living beings need many things in conjunction, so that they may be able by procreation to forge out the chain of the generations: first there must be food, next there must be a way for the life-giving seeds to ooze through the frame and be discharged from the body, and that male and female be joined they must both have the means to exchange mutual pleasures.
>
> And many species of animals must have perished at that time, unable by procreation to forge out the chain of posterity: for whatever you see feeding on the breath of life, either cunning or courage or at least quickness must have guarded and kept that kind from its earliest existence; many again still exist, entrusted to our protection, which remain, commended to us because of their usefulness. Firstly, the fierce brood of lions, that savage tribe, has been protected by courage, the wolf by cunning, by swiftness the stag. But the intelligent dog, so light of sleep and so true of heart, beasts of burden of all kinds, woolly sheep also, and horned herds of oxen, all these are entrusted to man's protection. For these have eagerly fled from the wild beasts, they have sought peace and the generous provision gained by no labour of theirs, which we give them as the reward of their usefulness. But those to which nature gives not such qualities, so that they could neither live by themselves at their own will, nor give us some usefulness for which we might suffer to feed them under our protection and be safe, these certainly lay at the mercy of others for prey and profit, being all hampered by their own fateful chains, until nature brought that race to destruction.

Thanks to Lucretius, the ability of nature to preserve certain types and eliminate others was announced clearly in the first century B.C. It would seem that a mature conception of natural selection might have developed as a logical consequence of such a promising start. Lucretius, however, was a mechanist. His philosophy tended to minimize the personal activity of the Gods; so, of course, it was received with hostility by the revealed religion of his day.

The soon-to-emerge Christianity took over the perquisites of the older faiths and, becoming in turn the revealed religion, continued to be hostile to Lucretius. Like the earlier faiths, it looked upon him as a man who sought to limit the power of God. The hostility of the Christians to Lucretius and to his rationalism was expressed vividly by Lactantius (260–340 A.D.) some three-and-a-half centuries later. (See pp. 61 ff.)

Strangely enough, Lucretius devised another hypothesis that met with general acceptance late in the nineteenth centry, the hypothesis of the "continuity of the germ plasm." He described a continuous germ plasm composed of unaltered particles passing down through the generations. It was the existence of these particles that explained how characters and traits of the more distant ancestors could reappear after they had been hidden for a generation or so. (From Bk. V, lines 1217–1228):

> If often happens also that the children may appear like a grandfather and reproduce the looks of a great-grandfather, because the parents often conceal in their bodies many primordia mingled in many ways, which fathers hand on to fathers received from their stock; from these Venus brings forth forms with varying lot, and reproduces the countenance, the voice, the hair of their ancestors.

Lucretius was the first to record a disbelief in the inheritance of acquired characters (from Bk. V, lines 1038–1049):

> . . . For if the first beginnings of things could be changed being in any way overmastered, it would also now remain uncertain what could arise and what could not, in a work in what way the power of each thing is limited and its deep-set boundary mark, nor could the generations so often repeat after their kind the nature, manners living and movement of their parents.
>
> As soon as the seed comes forth driven from its retreats, it is withdrawn from the whole body through all of the limbs and members, gathering in the fixed place in the structure and arouses at once the genital parts alone. Those parts thus excited swell with the seed and there is a desire to emit it toward that whither the dire craving tends; and the body seeks that which has wounded the mind with love.

In spite of this rejection of the inheritance of acquired characters, Lucretius, as we can see, visualized the hereditary mechanism as including a limited sort of pangenesis.

In spite of his biological prescience, Lucretius' chief interests were not biological, or rather, they were biological only in part. He was a philosopher but one who had the true scientific instinct. He rejected the supernatural and did his best to understand the natural. He had a marked influence upon the sophisticated minority of his

time—on Cicero in particular and also on the agnostic Julius Caesar—but his hostility to all religion limited his popularity and influence. Lactantius buried him very effectively, and he is only now coming into his own. Perhaps no philosopher has ever been slandered more. It has been said that his character was so vile that he became unable to live with himself, so he committed suicide.

With Lucretius, the Roman, we bring our Hellenistic Age to a close, but this Latin ending to a Grecian age is not the paradox it might seem. The ideas that Lucretius expressed so eloquently were but amplifications of Greek philosophy. He was a follower of Epicuros and we can recognize very easily the gifts he received from Empedocles. Beginning with the next century, the first century after Christ, a combined Greco-Roman culture arose—the culture of the middle period of the Hellenistic-Roman era. In this century the philosophers and physicians, who recorded the current biological beliefs, were both Greeks and Romans. This was a century in which no real advances in biology were made in spite of the fact that there was considerable activity in the biological fields. Such well-known figures as Celsus, Dioscorides, and Pliny did their bit to keep biology alive, but the coming retreat from scientific standards was already foreshadowed. In the following century—the second after Christ—the deterioration of scientific standards would be out into the open for everyone to see. Celsus and Dioscorides, however, wrought nobly but in a losing cause.

Aurelius Cornelius Celsus, who flourished under the emperor Tiberius (whose reign lasted from 14 to 37 A.D.), was an encyclopaedist. He wrote on rhetoric, philosophy, law, military science, agriculture, and medicine. Only his eight books on medicine have survived. This work is an excellent compilation, well written and well organized. It is in fact the best account of the medicine of classical times next to those of Hippocrates and Galen. It contains many anatomical details and some excellent descriptions of contemporary surgery, such as operations for cataract, removal of tonsils, and dental and plastic surgery. For a while all of the work of Celsus was lost, but his medical books were referred to often in the fifteenth century and their impact on the medicine of the time was tremendous. Their great influence in the Renaissance was due not only to their merits but also to the historical accident that they were printed some time before the works of Hippocrates and Galen.

The name of Pedanios Dioscorides is well known to pharmaceuti-

cal botanists. He was a Greek who came from Cilicia, not far from the city of Tarsos, and he flourished around the middle of the century. He wrote a materia medica, an encyclopaedia of medicinal plants. In it he described and pictured 600 specimens. This compilation embodied the results of practically all of the Greek research on medicinal plants and was far more complete than any earlier compilation. It was also beautifully illustrated and, remarkably enough, the illustrations have come down to us by way of a Byzantine manuscript that can be dated about 512 A.D. For just over 1,000 years this manuscript remained in Constantinople, but it is now in the Imperial Library in Vienna. Facsimile reproductions have made it available to students in general.

The historical importance of Dioscorides is much greater than it would appear to be on the surface. The illustrated manuscript of his work became available for study in the sixteenth century, just when the herbals were being printed. Dioscorides became *the* authority on medicinal plants, and several of the herbalists presented their own work merely as commentaries on that of Dioscorides. Others claimed that they founded their work on his. His fame and importance were undiminished throughout the seventeenth century. In fact, Dioscorides is as important in the history of medicinal botany as Theophrastos is in the history of botany in general, and Dioscorides is far more important in the history of botanical iconography.

If the history of science were only a listing of scientific discoveries, with the dates at which they were made and the names of the scientists who made them, we could and perhaps should omit the name of Gaius Plinius Secundus (23–79 A.D.). Pliny had only a few of the qualities that we believe all good scientists should have. He was credulous, uncritical, and almost incapable of discriminating between the reasonable and the unreasonable, or even between the important and the trivial. He was superstitious, sophomoric and dogmatic—a true representative of his age and time.

He possessed, however, his full quota of the virtues. He was conscientious, courageous, and almost unbelievably industrious. He had an overwhelming sense of duty to his fellow men and to the Emperor of the Roman world. He belonged to the provincial nobility and he possessed a fair amount of wealth. He accepted without questioning the obligations of his birth and, whenever called upon, he was willing to serve the state in any capacity and as a matter of course. He was the type specimen of the man who believed in and

maintained the somewhat old-fashioned and aristocratic standards of the gentleman. His influence on the history of science, and his role in the transmission of knowledge from the classical world to Renaissance Europe were tremendous. For fifteen hundred years, he was one of the four great authorities, ranking with Aristotle, Virgil, and St. Augustine in the esteem of the medieval Latin world.

Pliny deserves more space perhaps than we can devote to him here, so we will mention only those aspects of his life and works that are of major significance in the history of biology. His importance to us lies not so much in the biology he transmitted as in the fact that it was a man like Pliny who transmitted it. As we have stated earlier, biology did not become a science through the activities of professional biologists. It was merely an incidental consequence of the activities of physicians, horticulturists, philosophers, and, at times, of gentlemen amateurs. Biology developed as a peripheral and unplanned growth within the slowly expanding sphere of knowledge.

Pliny was a historian, a lawyer, an admiral, and a cavalry commander who served on the German frontier. Later he was the comptroller of the revenues of Spain. He was also the friend, adviser and confidant of the Emperor Vespasian. He was in command of the Roman fleet that attempted to rescue the people who were fleeing from the great eruption of Vesuvius. It was while he was engaged in this task that he died of a heart attack, although the uninformed tradition has it that he was overcome by the fumes of Vesuvius.

Of his voluminous writings, only the thirty-seven books of his *Natural History* have survived. This work, a great encyclopedia, was but a small fraction of his total output.

Pliny's life and death well deserved our admiration. He was a great individual, but he was by no means a great biologist, yet for more than a thousand years he was the chief authority on biology in Latin Europe. His *Natural History* was truly an encyclopedia, and over a fourth of it was devoted to biological topics. Book VII covered anthropology, Books VIII to XI zoology, and Books XII to XVII botany. Pliny's botany was chiefly medical and agricultural but he mentioned more plants than did Dioscorides. His biology, however, was second-hand and completely uncritical. He was much too busy to be discriminating.

Pliny's zoology was fanciful and founded on hearsay, yet this very fact tells us a great deal about the scientific standards of the times

and about the biological beliefs of the educated, cultured, and politically important Romans. His *Natural History*, however, contains a great deal of authentic information. The single quotation that follows shows how far it may also go in recording nonsense (Bk. VIII, chap. 1, § 3, tr. by H. Rackham):

> The largest land animal is the elephant, and it is the nearest to man in intelligence: it understands the language of its country and obeys orders, remembers duties that it has been taught, is pleased by affection and by marks of honour, nay more it possesses virtues rare in man, honesty, wisdom, justice, also respect for the stars and reverence for the sun and moon. Authorities state that in the forest of Mauretania, when the new moon is shining, herds of elephants go down to a river named Amilo and there perform a ritual of purification, sprinkling themselves with water, and after thus paying their respects to the moon return to the woods carrying before them those of their calves who are tired. They are also believed to understand the obligations of another's religion in so far as to refuse to embark on board ships when going overseas before they are lured on by the mahout's sworn promise in regard to their return. And they have been seen when exhausted by suffering (as even those vast frames are attacked by diseases) to lie on their backs and throw grass up to the heaven, as though deputing the earth to support their prayers. Indeed so far as concerns docility, they do homage to their king by kneeling before him and proffering garlands. The Indians employ the smaller breed, which they call the bastard elephant, for ploughing.
>
> . . . .
>
> It is surprising that they can even climb ropes, but especially that they can come down them again, at all events when they are stretched at a slope. Mucianus who was three times consul states that one elephant actually learnt the shapes of the Greek letters, and used to write out in words of that language: "I myself wrote this and dedicated these spoils won from the Celts" and also that he personally had seen elephants that, when having been brought by sea to Pozzuoli they were made to walk off the ship, were frightened by the length of the gangway stretching a long way out from the land and turned around and went backwards, so as to cheat themselves in their estimation of the distance.

Such quotations as the above could be repeated many times over although they would not give the complete picture or be completely fair to Pliny. However, our emphasis on Pliny's credulity is deliberate because his gullibility illustrates a change that was slowly coming over the scientific world.

Merely to mention the more striking instances of Pliny's credulity would require too much space. He endorsed practically every traveller's tale that was current during his life. Some of the stories, of course, were true, and he told of the Carthaginian, Hanno, who had sailed down the west coast of Africa and brought back the skin of a

"hairy woman"—probably the skin of an anthropoid ape. But he told also of queer and impossible monsters. For example, he described the offspring of the promiscuous hybridization that supposedly occurred routinely in Africa, and which he got from Aristotle. He also told how the eel crossed with the viper and how the lioness hybridized with the panther. Nor did he omit the unfortunate consequences of human-animal crosses. He stated (Bk. VII, chap. 3) that a woman once gave birth to an elephant and that another gave birth to a snake. In Thessaly, he stated that a creature was born who was half man, half horse, but that it soon died.

Pliny's credulity was not a mere personal idiosyncrasy. If it were, it would not be worth mentioning. His beliefs and intellectual standards were typical of his period. They were not only common to the usual run of Roman citizen, but they were also held by the learned minority. To his contemporaries, Pliny was a learned man, a scholar, as well as a prominent and respected public official. He was also the great classical authority during the medieval period and on down to the Renaissance.

The intellectual standards within the Roman Imperium were now deteriorating. Scientific knowledge was being forgotten and some of it would be lost. A virulent anti-intellectualism was growing unhindered, one that would shortly supplant the earlier skeptical and critical thinking of the classical world. The "wave of the future" was submerging all scientific standards. New and vigorous superstitions were spreading, the social structure was weakening, and, after a few more generations, the barbarians would overrun and destroy the still civilized but decadent Mediterranean world. The Latin nations of the West would be the first to go.

We should not ignore such periods as this in the history of science, because the growth of science has always been irregular and erratic. It cannot be described by any simple exponential or parabolic curve, guaranteed to reach infinity in the near future. As we know, all growth curves are sigmoid, although in their earlier phases the point of flexion may not be at all obvious. Historians of science have frequently stated that the history of science is the only history that is truly cumulative. It is well to emphasize—and here is a good place to do it—that this is not the whole story, that past accumulations have often been lost, and that the brainpower of a culture may sometimes be inadequate to preserve what past generations have

learned. With the exception of Galen, none of Pliny's successors who wrote on biological topics attained even Pliny's standards. Let us pass now to Oppian.

Oppian's name should be included in the history of biology but thus far it seems to have been overlooked. This is a double oversight because there were two Oppians, who were fused, perhaps deliberately, into a single entity. The first Oppian came from Cilicia and flourished during the reign of Marcus Aurelius (161–180). He wrote a long poem of 3,500 lines on fishing, *Halieutica*. The second Oppian lived nearly a century later and came from Syria. He wrote a poem of 2,150 lines on hunting, *Cynogetica*.

These poems contain accurate data on the characteristics and habits of fish and of animals in general and also a respectable amount of incidental biological information, such as what we should expect when we cross different breeds of dogs. They also describe a number of impossible and fantastic hybrids, the extreme being the description of the ostrich as a cross between the sparrow and the camel. The pre-natal influence of the mother's imagination is accepted as self-evident, as is the influence of the mother's milk on the character of the child. These fantastic notions give us a clear picture of the biology of the second century after Christ.

The *Physiologus* also dates from the latter half of this century. It was composed originally in Alexandria, but much was added to it later. It is a collection of Christian allegories based upon the marvelous peculiarities of animals. It is much like Aesop's *fables*, but it was not considered fabulous by those for whom it was written. For a thousand years, it was used in sermons throughout Christendom to illustrate morals and teach religious lessons. It was translated from the original Greek into Ethiopian, Armenian, Syriac, Latin, Arabic, Old High German, Anglo-Saxon, Icelandic, Rumanian, Provençal, Old French, Slavic, etc.

The *Physiologus* shows how completely zoology could be degraded even at a time when valid zoological knowledge still existed in more intellectual circles. The following citations from it are typical: when the lion smells the hunter, he sweeps away his tracks with his tail so the hunter will not find him. The lion is also born dead, but the cubs are revived on the third day. The "ant-lion" is a hybrid between the ant and the lion. Its fore part is lion and can eat only meat, its back parts are ant and can digest only vegetables. This combination does not work, so: "The Ant-Lion perishes for lack of

food" (Job 4:11). When hunted, the beaver castrates himself and tosses his testes back where the hunter can find them. As the testes are what the hunter is really after, he takes them, and allows the beaver to escape. The unicorn can be caught only by a pure virgin. He puts his head in her lap and will follow her home. If, however, she is a virgin in name only, he will kill her with his horn. The elephant has no joints in its legs and when it falls down it cannot get up again. The female asp bites off the head of the male and the young asps gnaw off their mother's head. The echinus is a small fish that can attach itself to the largest ship and hold it stationary in the water. The hyaena is a hermaphrodite—one year he is male, the next year she is female.

We can readily see from the above how the *Physiologus* could illustrate many sermons and how natural history could be used to expound and illustrate scripture.

The name of Lucius Apuleius, born about 125 in Numidia, belongs in the history of biology only through an accident. His own scientific writings are lost, but he has been confused with another Apuleius —Apuleius Platonicus or Barbarus, or Pseudo-Apuleius—who lived at the end of the fourth or the beginning of the fifth century. The second Apuleius wrote *De Herbarum Virtutibus,* a very early herbal. It was printed in 1481 and contains the first printed illustrations of plants. These illustrations in manuscript may even be copies of those of Cratevas. Pseudo-Apuleius is important in the history of botanical iconography and in the transmission of knowledge concerning medicinal plants. The writings of Lucius Apuleius that still exist include a book on magic and the famous *Golden Ass,* or *Metamorphosis.*

While the intellectual level of the classical world was deteriorating, valid information was still available for the instruction of any first-rate man who sought to master a science. Indeed, it was at this time that one of the greatest authorities in the field of medicine achieved his eminence. He was Galen (129–200) of Pergamon. Next to Hippocrates, Galen was the greatest physician of antiquity. He dissected many animals—some while they were living, but his knowledge of human anatomy was faulty. This was to have serious consequences in the sixteenth century, when Vesalius reformed anatomy, and the great conflict that arose then was between those who accepted Galen's descriptions of human organs and those who followed Vesalius. Before 1543, Galen was the great authority in

the medical sciences and he deserved his rank. It took a Vesalius to surpass him.

Galen made and recorded a number of new facts in the fields of anatomy, physiology, pathology, embryology, therapeutics, and pharmacology. He studied the mechanism of respiration and the functions of the kidneys, the cerebrum, spinal cord, etc. He proved experimentally that the arteries carried blood and "pneuma" and he expressed the hope that "pneuma" would one day be discovered—oxygen was!

Galen's writings were numerous, and very clear and complete. They were translated into Arabic as well as into Latin and, after the splitting of the Mediterranean world into its Latin and Greek halves, his views in the field of medicine dominated them both. Today we tend to emphasize Galen's weaknesses and his deficiencies rather than his virtues, and his deficiencies were numerous. He was dogmatic and had much too much confidence in the deductive method. He did not believe that the environment ever altered the structure of an animal but that all organs had been designed to be just as they were—each organ served a purpose. If someone were to ask, for instance, why a man did not have the long, effective ears of the donkey, the reply would be that his ears are small so that he could wear a hat. Galen was an Aristotelian, a vitalist, and a complete teleologist. His work was essentially beneficial for the first few centuries after it appeared but, ultimately, it tended to block progress.

We will cite but two more individuals whose lives overlapped Galen's. Claudius Aelianus was born in Italy and flourished from 193 to 211. He was a moralist who, among his numerous writings, compiled a work in 17 books on the *Peculiarities of Animals*. This zoology was anecdotal, highly moral, and much like Aesop's fables. It was definitely pre-Christian, for when the animals were in trouble they relied upon the pagan deities for succor. But by changing the names of the gods to those of the saints, it could be made a useful source for Christian sermons. Aelian did transmit some sound knowledge, however, along with many of the current myths and the strange beliefs about the animal kingdom.

Maximus of Tyre was a philosopher who lived during the latter half of the second century. He published a number of dissertations, and in one of them (Dissertation XXII) he described the precise adaptations of animals to their habitats and living conditions. He came close enough to the concept of natural selection to record the

fact that unadapted animals perished and became extinct. Thus (from Dissertation XXII):

> As horses, therefore, for their safety are allotted the race, oxen labour, birds wings, lions strength, and other animals something else; in like manner a connate power which preserves the race is present with man. With respect to this power, it is necessary that it should be different from that of other animals; if, being man, he is to be saved, not by strength as lions, nor by the race as horse, and is not to carry burthens like an ass, nor to plough like an ox, nor to fly with birds, nor swim with fishes. But there is also a certain work peculiar to this animal, which preserves his life, if powers are distributed to animals according to the use of life, works according to powers, and instruments according to works, and the good they effect.

The above quotation, together with some other scattered excerpts from Maximus, show that he came close to getting a notion of natural selection; but apparently he influenced no one, and the notion itself was becoming violently unpopular. The idea that any mechanism might account for the adaptations of organisms to their environment was beginning to be looked upon not only as an error but also as a sin, especially among the Christians, who were growing in influence and who would soon acquire enough political power to liquidate all competing religions.

The final period of the Hellenistic-Roman era may be called the "Christian," although Christianity did not become the official religion of the Roman Empire until the fourth century. The influence of Christian thinking, however, had become manifest a century earlier. The general tolerance and skeptical thinking of the pagan world were vanishing. Correct belief and faith now were held in high esteem; in fact, they were soon incorporated in the better thought of virtues. Theology had become the most revered and, politically, the most important of the sciences. The secular and imaginative speculations of the earlier Greeks became not only unfashionable but also a little dangerous. In an earlier age several of the Greeks, and such Romans as Lucretius, had made bold, if not very successful, efforts to explain existence rationally and in terms of natural processes. But from the fourth century on and as the Christians grew in influence, all such efforts ceased. Lucretius became a major villain, and supernaturalism became the only moral and allowable explanation as to how things had come to be as they were. This brings us to the *Divinarum Institutionum* of Lucius Caelius Lactantius (*Ferminianus*) who lived from about 260–340.

Lactantius, a well-educated Roman, became converted to Chris-

tianity. He wrote the *Divine Institutes* between 304 and 311. He was not a biologist and he seems to have been omitted from most histories of the science. However, he did express clearly and emphatically the philosophical line that would dominate the science until the time of Darwin. The following quotations describe precisely the philosophical orientation of biology that would be dominant until the middle of the nineteenth century (from chap. 6, *On the Workmanship of God*):

"I cannot here be prevented from again showing the folly of Epicurus. For all the ravings of Lucretius belong to him, who, in order that he might show that animals are not produced by a contrivance of the divine mind, but, as he was wont to say, by chance, said that in the beginning of the world innumerable other animals of wonderful form and magnitude were produced; but that they were unable to be permanent because either the power of taking food, or the method of uniting and generating had failed them. It is evident that, in order to make a place for his atoms flying about through boundless and empty space, he wished to exclude the divine providence. But when he saw that a wonderful system of providence is contained in all things which breathe, what vanity was it (O mischievous one!) to say that there had been animals of immense size, in which the system of production ceased.

"Since therefore, all things which we see are produced with reference to a plan—for nothing but a plan can effect this very condition of being born—it is manifest that nothing could have been born without a plan. . . . Why should any one suppose that, in the contrivance of animals, God did not foresee what things were necessary for living, before giving life itself? For it is manifest that life could not exist unless those things by which it exists were previously arranged.

"Therefore Epicurus saw in the bodies of animals the skill of a divine plan, but that he might carry into effect that which he had before imprudently assumed, he added another absurdity agreeing with the former. For he said that the eyes were not produced for seeing, nor the ears for hearing, nor the feet for walking; but that all the offices of these members arose from them after their production. I fear lest the refutation of such extravagant and ridiculous stories should appear to be no less foolish; but it pleases me to be foolish, since we are dealing with a foolish man, lest he should think himself too clever. What do you say, Epicurus? Were not the eyes

produced for seeing? Why, then, do they see? Their use, he says afterwards showed itself. Therefore they were produced for the sake of seeing, since they can do nothing else but see. Likewise, in the case of the other limbs, use itself shows for what purpose they were produced. For it is plain that this use could have no existence, unless all the limbs had been made with such arrangement and foresight that they might be able to have their use.

"For what, if you should say, that birds were not made to fly, nor wild beasts to rage, nor fishes to swim, nor men to be wise, when it is evident that living creatures are subject to that natural disposition and office to which each was created? But it is evident that he who has lost the main point itself of the truth must always be in error. For if all things are produced not by providence, but by a fortuitous meeting together of atoms, why does it never happen by chance, that those first principles meet together in such a way as to make an animal of such a kind, that it might rather hear with its nostrils, smell with its eyes, and see with its ears? For if the first principles leave no kind of position untried monstrous productions of this kind ought daily to have been brought forth, in which the arrangement of the limbs might be distorted and the use far different from that which prevails. But since all the limbs observe their own laws and arrangements, and the uses assigned to them, it is plain that nothing is made by chance, since a perpetual arrangement of the divine plan is preserved. But we will refute Epicurus at another time." (From chap. 2):

"Since he did not give that power of reason to the other animals, He provided beforehand in what manner their life might be more safe. For He clothed them all with their own natural hair, in order that they might more easily endure the severity of frosts and cold. Moreover, He has appointed to every kind of its own peculiar defense for the repelling of attacks from without; so that they may either oppose the stronger animals with natural weapons, or the feebler ones may withdraw themselves from danger by the swiftness of their flight, or those which require at once both strength and swiftness may protect themselves by craft, or guard themselves in hiding-places. And so others of them either poise themselves aloft with light plumage, or are supported by hooves, or are furnished with horns; some have arms in their mouth—namely, their teeth—or hooked talons on their feet; and none of them is destitute of a defense for its own protection.

"But if any fall as a prey to the greater animals, that their race might not utterly perish, they have either been banished to that region where the greater ones cannot exist, or they have received a more abundant fruitfulness in production, that food might be supplied from them to the beasts which are nourished by blood, and yet their multitude might survive the slaughter inflicted upon them, so as to preserve the race."

Nature might indeed be red in tooth and claw but that seems to have been what God wanted. Pre-ordained victims should be able to breed copiously! Teleology had come into its own. Lactantius also defended his belief that the world was flat.

From the middle of the fourth century on, the intellectual life of the Mediterranean world had passed into the custody of the Christians. The little learning that was preserved in the Latin countries and ultimately transmitted to us was preserved and transmitted by the Christians. In the eastern, or Grecian, half, the pagan scholars and philosophers were beginning to disappear. But except for such incidents as the destruction of the Museum at Alexandria their works would not be lost—only allowed to deteriorate through neglect. Theology was absorbing more and more of the attention of the learned, and the scientific knowledge that remained was being supplemented with—and even supplanted by—dogma. Dogma at least gave definite answers to the philosophical questions that could be asked. New discoveries in biology simply were not made, and the biology that persisted was preserved only incidentally and in works on other subjects.

The history of the world was being fitted into the framework of the ancient Hebrew writings, and the origin of the animals and plants was being traced back to the creation of the world as described in Genesis. Commentaries on the days of creation, however, contained pronouncements on scientific topics, and we can tell from them just what the current views were on matters biological. We shall cite but two of these commentaries. The first is from the *Hexaemeron* of St. Ambrose of Milan (339 or 340–397). St. Ambrose was born in Treves, educated in Rome, and baptized in 384. He served as Bishop of Milan (374–397). Intellectually he was vigorous though dogmatic, and he became a major influence in the Western Church. The biology that we can extract from his work is merely the biology of the time and it contains the usual run of vulgar errors. For example he quoted as fact the belief that female

vultures were impregnated by the south wind and he held that this fact made the virgin birth of Christ more reasonable. He did describe the two sexes of the date palm, but he missed the significance of pollen. He mentioned incidentally that cultivation would ameliorate wild plants.

St. Basil the Great (331–379) also composed a work on the first chapter of Genesis. He was born at Caesarea and served as Bishop there from 370 to 379. His work parallels St. Ambrose's in many ways. In his *Hexaemeron* he described the sexual reproduction of the palm and fig trees. The six days of creation, listing as they did the organic forms in the order of their complexity, seemed to furnish an excellent framework for an exposition of the cosmos. They provided a splendid guide for the presentations of topics in the great encyclopedias that were being composed, some as late as the thirteenth century.

With St. Augustine, Bishop of Hippo (354–430), we come to one of the keenest minds of antiquity. He was born in Numidia and was well educated in pre-Christian philosophy. He became in succession a Manichaean, a Neoplatonist, and a Catholic. He was a major factor in shaping the thought of the West. Even today his views carry weight, especially those that impinge upon evolution. His biological pronouncements are scattered through his numerous writings and are in general like those of his contemporaries. For example, he cited a number of wonders that no one could explain in order to justify a belief founded purely on faith, but he also cited numerous isolated bits of sound biology—for example, that olives did not breed true from seed, but had to be propagated by grafts.

He believed the impressions a mother received would be transmitted to her embryo (*On Marriage and Concupiscence*, Bk. XII, chap. 23) but not that such acquired characters would be passed on indefinitely. He was impressed by the fact that occasionally monsters were produced—men with six fingers, etc.—and that individuals and species did not always breed true. To this extent his views were in harmony with our present concept of evolution. He held, however, that the higher animals had been created directly by God, and he was especially emphatic in his statements that the human soul was God's own handiwork. He believed that all men—all human races —regardless of their physical differences and climatic adaptations, were descended from Adam. God gave the human species this origin so that all men would know that they were brothers.

In the broader sense, however, St. Augustine's concept of *potential* creation fits in well with the modern theory of evolution. He conceived of matter as having been so created that it had the property of bringing forth life, just as the circumstances or the occasion might demand. He believed in spontaneous generation, and in the successive appearance of some of the lower species. He described how certain forms came into being: some species "smaller even than mice or lizards, such as locusts, beetles, flies, and even fleas might not have been in Noah's ark . . . some animals, like flies are not born but spring from any kind of decaying matter and only later mate and procreate, and that others like bees, have no sex at all." (Bk. XV, chap. 27.) He was of the opinion that if we did not know that apes, monkeys and baboons were not human, some historians would claim that they were. Many of the curious races that exist, he held, such as pygmies, the cynocephalae—the dog-headed men who barked—are not necessarily human. But all *rational* creatures, no matter how odd their appearance, have souls and are descended from Adam. (Bk. XVI, chaps. 7–10.) In spite of all such odd creatures, however, St. Augustine was convinced that our present world was only 6,000 years old. He could not be considered an evolutionist in the modern sense of the term. Nevertheless, in many respects his beliefs are compatible with evolution.

We shall cite but one more scholar who lived during the period when the Latin world was taking its long vacation from scholarship and from scientific standards. He was St. Isidore of Seville (*ca.* 560–636). His numerous writings are of great value for historians of science for he covered a great deal of ground and he did transmit some scientific knowledge. The twenty books of his *Etymologiarum* and his *De Natura Rerum* also show his keen interest in science. To him science had an importance of its own apart from theology.

4

A Bucolic Interlude

The Arcadian joys of country living have always had a sentimental appeal to those who have never had to work on a farm. Those who have, generally try to escape to the city at the first opportunity, and this behavior has been routine ever since cities were invented.

In real life, shepherds and shepherdesses do not wander around clothed in silks, nor do they spend the greater part of their time dancing to the music of flutes. Ever since the plow was invented, agriculture has required enormous amounts of hard work, primarily because until recently its yields have been very small. In Roman times, the yield of barley and wheat in Italy was sometimes as low as four fold. How meager this increase is can be shown by comparing it with the better returns of today. The best yields of maize in the corn belt of the United States average sixty fold, while wheat, when grown on irrigated land, has yielded as much as seventy fold. In a moist year in the wheat belt of the United States where the farming is mechanized rather than intensive, where there is no crop rotation and the land does not "rest" between crops, and where mineral nutrients are not added to the soil the yield usually falls between fifteen and twenty fold.

The low returns on agricultural labor before the middle of the nineteenth century should be emphasized because our present agricultural surpluses are apt to make us forget how our ancestors had to struggle to get enough to eat, and how often they failed. It was only in the middle of the nineteenth century that Liebig identified for the first time the mineral nutrients that plants needed for their

growth, but it was not until the twentieth century that we discovered that plants also need small amounts of the trace elements. However, these discoveries are recent. They were made only yesterday. For ages, even the fact the plants needed specific elements was unknown.

At this point, perhaps, it might be well to consider the discoveries that were not made by the earlier agriculturalists, who unfortunately knew almost nothing about what went on inside the plant. Plant physiology is, in fact, a very recent science. For only a little over a hundred years have we had any knowledge of the basic principles of soil fertility. The prehistoric agriculturists had little or no understanding of what soil fertility consisted of, although they had done a truly excellent job in developing the plants that were worth cultivating. They succeeded best in the river valleys of Egypt and Mesopotamia where the fertility of the soil was never a problem, though even in Mesopotamia some of the irrigated land became so impregnated with salt that it had to be abandoned.

This must not be taken to mean that classical agriculture contained no directives for adding to the fertility of the fields. Some of the empirical methods of fertilizing are older than history, and indicate that the earlier farmers had experimented widely; but even so, they routinely exhausted all of the land they cultivated. Some of the experiments, however, paid off. As we have stated in Chapter 1, the American Indians learned that if they buried a fish in each hill of corn, their crop would be fertilized, though they did not know why. We can be certain that it was not because the fish god pushed the corn up through the ground. Everywhere that crops were grown, the farmers discovered that if either animal or human excrement was spread upon a field, its exhaustion would be delayed. Wood ashes were also recognized as a good fertilizer and, as early as Roman times, certain types of soil were improved by the addition of marl, a clay that contains calcium carbonate.

When history began, the agriculturists had already learned that their fields needed a "rest" and that the fields would recover some fertility merely by lying fallow. By Roman times a type of crop rotation had been invented, in which a legume such as clover, peas, or beans was grown in a field that had been planted with grain the preceding year. During the third year, the field was often used as a pasture, and not until a year later would it be again planted in barley or wheat. This three-year system of rotation is not bad; indeed it would not be greatly improved on until the eighteenth cen-

tury, and not really supplanted until the nineteenth. Classical agriculture would have been fairly effective if it had not been for erosion.

Erosion was the great enemy, and it was an enemy that had many allies. Its chief ally was the domesticated goat. The goat gnawed the vegetation down to a point where it could not delay the run-off of the seasonal rains. As a result the sloping hillsides were often denuded of soil. Even the forested mountains where goats grazed —the mountains in Lebanon, in North Africa, and even in Italy itself—lost so much soil that they could no longer support their valuable stands of timber. Sheet erosion was also widespread in the cultivated fields and there is little doubt that the classical world experienced a slow and uninterrupted decline in agricultural production. This took place in spite of the fact that the Romans were intensely interested in agriculture, both as a science and as a basis for their social order. The Romans knew much more about growing plants and raising animals than they did about their soils.

From the very beginnings of agriculture, directives for proper farming must have been handed down from generation to generation by word of mouth. The very efficiency of the oral tradition made written precepts and admonitions unnecessary. Some few writings exist, though, and recently, S. N. Kramer found and translated a Sumerian "Farmer's Almanac" dating from about 1700 B.C. It begins: "In days of yore a farmer gave these instructions to his son: . . ."

A thousand years later, Hesiod (*ca.* 700 B.C.), who lived in Boeotia, composed his famous *Works and Days.* This was a great didactic poem, and because it was in verse, it was easy to memorize. Hesiod also wrote the *Theogony,* describing the descent of the gods, and a treatise on astronomy of which only fragments remain.

While the *Works and Days* is short—only 828 lines—it covers a lot of ground. It contains rules for husbandry and for navigation, ethical precepts, and a list of lucky and unlucky days. One passage hints at the possibility—astonishing to us—that the ancient agriculturists, as well as the first navigators, had to know the stars; the navigators needed them to steer by, and the farmers to know when to plant ("When the Pleiades, daughters of Atlas, are arising, begin your harvest; when they are going to set, begin your plowing. Forty nights and days they are hidden and appear again as the year moves round, when first you sharpen your sickle.").

Hesiod had a full appreciation of the joys of country living and

of the satisfaction that follows work well done. His practical agricultural precepts are sound but he was also as superstitious as some of our modern farmers who try to plant their crops in the proper phases of the moon. He held, for instance, that the twelfth day of the month was auspicious for emasculating mules, the fourteenth for training them, and the twenty-ninth for yoking both mules and oxen. His *Works and Days* was a major source for all the classical writers on agriculture who followed him.

Some centuries passed, however, before any classical writer followed Hesiod's example by writing on agriculture. But far to the west, and in another culture, Mago the Carthaginian wrote an extremely important agricultural treatise—so important in fact that, when it came into the possession of the Romans, the Roman Senate ordered it to be translated into Latin. We know nothing about Mago except that he wrote this work. We do not even know when he lived except that it was before 146 B.C., when Carthage was destroyed. The original, in Punic, has been lost, and so has the Latin translation. However, as we have stated earlier, Cassius Dionysios translated an abbreviated version of the Latin into Greek, and fragments of this translation still exist. The later Roman writers, Varro and Columella, praised Mago in somewhat extravagant language. Varro cited some fifty Greek writers on agriculture and then stated, "All these are surpassed in reputation by Mago of Carthage." Columella called Mago the father of agriculture.

One royal botanist should not be overlooked. He was Attalos III (171–133 B.C.), King of Pergamon from 138 to 133 B.C. His work on agriculture was used by the later Roman writers, though we have no indication that he himself made any important discovery. In addition to his bucolic contributions, Attalos III made an intensive study of poisonous plants and poisons in general. His interest in toxicology may have been objective and scientific in part, but certainly it was also very practical. A knowledge of poisons—and a supply of the better ones—has always been a great aid in disposing of inconvenient persons, and a proper knowledge of antidotes might well prolong the life of a king, since, as we have pointed out in Chapter 3, poisoning can often be an occupational hazard for royalty.

The earliest Roman work on agriculture is *De Re Rustica*, written by Marcus Porcius Cato (234–149 B.C.). It is also the oldest extant composition in Latin prose. Cato was a soldier who fought in the

Second Punic War, and also in Greece and Spain. He was a civil servant in Sicily, Africa, Sardinia, and Spain. He was a statesman and a moralist, and he served the Roman Republic as a censor. He was perhaps more of an agricultural economist than an agriculturalist proper, but he did describe farming, fruit growing, and gardening. He recognized the value of good plowing and of copious manuring. He described all the known forms of plant propagation, especially grafting, and he gave directions for transplanting trees, for viticulture, and for raising olives and figs. Of interest to systematic botanists is the fact that we still use the Latin names he gave his plants. Much of our nomenclature goes back to Cato.

Over a hundred years elapsed before the second great Roman agriculturist appeared. He was Marcus Terentius Varro (116–27 B.C.), one of the most learned of all the Romans. He was an encyclopedic writer but his chief occupation, as a Roman of good family, was public affairs. In his generation, this meant politics and war. He fought both in Spain and in Greece; he fought against the pirates and, as a naval commander, he made the mistake of supporting Pompey.

Varro's chief intellectual interests were in Roman history and the Latin language, and his writings on these subjects have added greatly to our knowledge of the classical world. He also wrote on rhetoric, arithmetic, music, medicine, astrology, and architecture, but most of these works are lost. At the age of eighty (36 B.C.) he wrote his *De Re Rustica*. Again we should emphasize the fact that the Romans who wrote on agriculture were chiefly men of affairs, and that their biological contributions were more or less incidental.

Varro knew more botany than Cato, possibly because the country estates of his time were much more complex than they had been in the preceding century, and many more plants were grown. The estates were real communities, and they were the scene of many of the activities later taken over by the villages and cities. Here, however, we shall be concerned with only the biological aspects of agriculture.

Varro included apiculture in his treatise. He knew as much about the honey bee as anyone else of his time. He did call the queen bee the king, but the sex of the queen was not established until 1669, when it was discovered by the Dutchman, Jan Swammerdam. Varro described the development of leaves and flowers, and the movements of plants.

He was interested in veterinary medicine and he seems to have had a vague notion of diseases being caused by microbes. We should not credit him, however, with creating the germ theory of disease, although there is a temptation to read some of our modern and painfully acquired knowledge into such passages as the following (Bk. I, chap. 12, trans. by Storr-Best):

> Note also, if there be any swampy ground, both for the reasons given above, and because certain minute animals, invisible to the eye, breed there, and, borne by the air, reach the inside of the body by way of the mouth and nose, and cause diseases which are difficult to get rid of.

In this passage, Varro was referring to malaria but, of course, he did not recognize the mosquito as the vector that carried the *Plasmodium* parasite.

Varro was especially interested in animal breeding, and he hybridized several varieties of pigeons. He even recorded one of the rare instances in which a fertile mule was bred. He was aware of the importance of heredity in the selection of farm animals for breeding and he warned against breeding a ram who had a black tongue because, he noted, such rams often begat black sheep. He stated that ewes that had once given birth to twins were valuable for future breeding. We might mention here that we now have varieties of sheep in which the ewes routinely produce twins.

As learned as Varro was, he was still susceptible to the general beliefs and superstitions of his time. He became interested in astrology but that is not relevant here. His biology also showed evidence of the general credulity of the period. He knew, of course, that hens will lay eggs even when they have had no contact with a rooster —lay the so-called "wind eggs." In fact he looked upon the wind as a general fertilizing factor—the air apparently bore germs of all kinds—and he repeated the generally held belief concerning the wind fertilization of the mares in Portugal.

Cato and Varro were prominent citizens and among the most influential men of their times. The next Roman to write on agriculture was one of the greatest men of all times. Publius Vergilius Maro (70–19 B.C.) is best known today for his epic poem the *Aeneid* —the poem that was to the Romans what the Homeric epics were to the Greeks. But Virgil died before the *Aeneid* was written in its final form—i.e., before it was published—and the reputation he enjoyed during his lifetime rested upon his *Bucolics* (42–37 B.C.) and his *Georgics* (37–30 B.C.).

Virgil was not only the greatest of Latin poets, he was also the

leading naturalist of his time. Inasmuch as the *Georgics* was composed in almost perfect Latin verse and was never lost or obscured, it helped to keep alive an appreciation of husbandry and a knowledge of agriculture, during the years when all learning was diminishing and when scholarship and science were in full retreat. Virgil was the one pagan poet universally honored by the Christians.

The sources of the *Georgics* are two fold: (1) Virgil himself was raised on a farm and knew by personal experience what he wrote about, and (2) he perused the ancient literature thoroughly and documented his poem by references to his Greek and Latin predecessors. He was a very learned farmer. The *Georgics* was divided into four books: Book I on agriculture in general; Book II on trees—especially fruit and olive trees—and on vines; Book III on stock farming; and Book IV on beekeeping. Virgil's account of beekeeping is perhaps the best that has come down to us from classical times. Virgil recorded, in language that has never been surpassed, the sum total of all the agricultural knowledge that existed. Philosophically, he showed the influence of Lucretius, but he lacked Lucretius' pessimism and he found the gods to be worth evoking in his pastoral verses. He also included some of the more popular errors of his time. He described the "degeneration" of species. Like Aristotle and Theophrastos, he believed that species were unstable units in nature but that their mutations unfortunately were, as a rule, degenerative. For example he reported that sometimes barley changed into wild oats. Such ideas prevailed until the eighteenth century. He also believed that almost any plant could be cross grafted with any other plant.

A century after Virgil, the most voluminous agricultural writer of the classical world appeared. He was Lucius Junius Moderatus Columella. He was born in Spain and travelled in Greece and Syria, but spent the greater part of his life in Italy. His *Res Rustica* consisted of thirteen books and, in its present form, it is an expanded version of an earlier work in three books. His *De Arboribus* is probably a book from this earlier work and, if so, is all that we have left of it. Columella naturally covered much of the botany and zoology of his time and he gave us a fairly accurate picture of classical biology—diluted by his detailed directions for successful farming. He had a practical knowledge of genetics, as is shown by his remarks on the breeding of cattle, horses, mules, and sheep, and he used his knowledge intelligently. He told how his uncle hybridized sheep to produce wool of different colors. He recognized the existence of

unit characteristics, and told how this same uncle had been able to combine the characters he wanted in the F_2 and subsequent generations.

The following quotation belongs in any history of genetics (Bk. VII, chap. 2, tr. by Curtius):

> Experience has also taught the way to produce other colors in this kind of animal. For when fierce wild rams of a marvelous color were brought across amongst other wild beasts from a neighboring district of Africa to the municipal town of Gades for those who were giving public shows, my uncle Marcus Columella, a man of keen intelligence and a distinguished agriculturist, bought some of them and transferred them to his estate, and when he had tamed them, mated them with "coated" ewes. These produced in the first generation lambs with coarse wool but of the same color as their sires. When these in turn were coupled with Tarentine ewes, they produced rams with a finer fleece. All the descendants of these latter, in their turn reproduced the soft wool of their dames and the colors of their sires and grandsires. Columella used to claim that in this way whatever outward appearance the wild animal possessed was reproduced in the second and later generations of their descendants, while their savage nature was tamed.

Columella described the use of hot beds covered with "transparent stone" wherein he could grow cucumbers the year round. In this connection he also endorsed a superstition of some importance. He stated that the touch of a woman was fatal to many growing vegetables, especially to those of the cucurbitaceae, and that a menstruating woman would spoil all such vegetables for human consumption merely by walking around the plot where they were grown. This belief persisted almost to modern times and caused considerable concern to English gardeners as late as the eighteenth century.

Columella's account of budding and grafting is by far the most detailed and complete that has come down to us from classical antiquity. He described practically every type of grafting that we use today. He stated specifically that cross grafting was possible and that it was not necessary for the plants to be alike in flower or fruit. He told precisely how it was possible to graft the olive onto the fig. He also gave detailed and accurate directions for securing chimeras in grapes—that is, sectorial graft hybrids—a technique that was endorsed by Francis Bacon in the seventeenth century. In this way he was able to produce grapes of different colors in the same bunch.

In Columella we find some of the best as well as some of the worst of classical biology. He gave the usual classical directives for controlling the sex of farm animals. By tying off the right or the

left testis of bulls, he claimed, they could be made to beget female or male offspring respectively. But with sheep this was too much trouble. Here if the rams were let into the ewes when the north wind blew they would beget males, but if the south wind blew, they would beget females.

Three centuries elapsed between the writings of Columella and those of the next and last important Roman agriculturalist. He was Rutilius Taurus Aemilianus Palladius, who flourished in the first half of the fourth century. His work, *De Agricultura,* in 14 books, consists mainly of a gardener's calendar that told the farmer just what he should do each month of the year. He also gave directions for draining swampy soil. His directions for maintaining the fertility of cultivated land were especially good and he recommended that seaweed should be used as a fertilizer whenever it was available. Seaweed is, of course, a good source of both potassium and iodine.

Palladius, like Columella, was greatly interested in the art of grafting, especially cross grafting, and he devoted an entire book (Bk. XIV) to the subject. Apparently he believed that the scion and the stock fused together and produced a compound plant. This, he thought, gave horticulturists a technique for producing new fruits by a form of hybridization. He said, for example, that when the citron was grafted on the pomegranate, it became a red citron (an orange?).

For us, the work of Palladius has an unexpected importance in that it was translated into Middle English. This translation, in the two printed versions, helps us follow the evolution of our language. The following quotation will illustrate both the writer's skepticism concerning the ability of the wind to control the sex of sheep and the language in which it was expressed (Bk. VIII, lines 95–99, tr. and ed. by B. Lodge, Early English Text Society):

> And toward that wynd yf the tuppis ofre,
> With litel malis fillith they the cofre,
> And with southwynd getith they femalis,
> Yf hit he sooth, right notable this tale is.

More than five hundred years elapsed between the writings of Cato and those of Palladius. This was a period of gradual, almost uninterrupted, intellectual deterioration. Mankind was progressing backwards. The "wave of the future" was inundating the depressing present, and the growing opinion was that, as far as this evil world was concerned, there would be no glorious future. The forward-lookers based their hopes upon the world they hoped to in-

habit after they died. Philosophy was beginning to bog down in countless theological minutia.

The science of agriculture, however, had an empirical factor, a safety factor that preserved it from a too rapid degeneration. By its very nature it had to remain practical, and this fact protected it from the doctrinaire aberrations that were contaminating so much of the current thinking. During this period many books on agriculture were written. Some were excellent compositions, and all were useful, although none of them described any important innovations. Soil erosion and soil exhaustion still presented unsolved problems.

Most of these works were lost. We know of them only through references in the writings that survived, or through fragments quoted in compilations that have come down to us. We shall bring this chapter to a close by describing one of these compilations, the *Geoponica* of Cassianos Bassos. Bassos was a Byzantine, who lived in the first half of the sixth century.

The *Geoponica* contains quotations from more than forty writers on husbandry. Some of those quoted were important and some were unimportant, but many of the writers are known to us only because Bassos quoted them. The following samples are chosen because they give the flavor of the compilations. Zoroaster (Pseudo-Zoroaster?) is quoted on the sympathies and antipathies of plants. Cassius Dionysios, who translated Mago, told how the right testis begot males while the left, females. He also wrote that when ordinary camels were crossed with swine they produce the two-humped, or Bactrian, camel. Julius Africanus, the great encyclopedist of the third century, recorded many of the current superstitions, and his contemporary, Florentinus, described the degeneration (change of species) that occurred routinely within the Brassica family of plants. He also gave directions for making citrons black by grafting them on apples and red by grafting them on pomegranates. Leontinus quoted Florentinus on the two sexes in the date palms. Diophanes (*ca.* 350) reported that the citron grafted on the pomegranate produced the orange. Didymos, who wrote late in the fourth century, compiled a *Geoponica* that was one of the chief sources used by Bassos. The *Geoponica* of Bassos was in turn the source of a later one compiled about the middle of the tenth century.

With Bassos we can bring our "bucolic interlude" to a close. The intellectual twilight had arrived.

5

Retrogression and the Beginnings of Recovery

The Dark Ages that followed the fall of Rome were not completely black, but they were deeply smudged. The intellectual twilight that had been settling over the Roman Empire grew steadily darker. The Empire itself split into its Latin and Greek halves and the two segments drew further and further apart. Ultimately, all intellectual contact between them was lost and the classical Roman world came to an end.

The cause of the decline and collapse of the mightiest empire the world had ever seen is one of the persisting puzzles of history. Likewise, the reasons for the deterioration of classical science, which preceded the political collapse by some centuries, are just as puzzling. The more we examine the decay of the scientific spirit which took place within an entire civilization the more perplexing it becomes.

The physical conditions seemed to have been almost ideal for the existence of a rapidly growing and healthy science. For the first time in history the Mediterranean world was united politically; it was enjoying one of its few periods of almost universal peace. Its inhabitants welcomed new ideas and their intellectual curiosity was strong. The ideas themselves were mobile; scholars and philosophers travelled wherever they wished. They also communicated with one another freely, because nothing in classical science was "classified." Schools, libraries, and even research institutions were

established and supported by the resources of an extensive and prosperous economy.

New concepts travelled quickly from one end of the Empire to another. There was nothing to stop them because the political and intellectual frontiers had been obliterated. The Mediterranean Sea was no longer a physical barrier; it had become an artery of commerce. On land, the Roman roads made travel easy, and Roman legions and administrators, Greek merchants, and simple tourists could journey quickly from any section of the Empire to any other. The centers of political and military power were also mobile, and their very mobility emphasized the unity of the classical world. For example, the Emperor Constantine, who founded Constantinople, received the imperial purple in the north of England—in the city of York.

Information and ideas, ethical systems and religions, mysteries and cults—all diffused freely from one country to another. Tolerance was universal. Freedom of conscience, or belief, and of speculation was unquestioned. The world was inhabited by highly civilized and sophisticated people. Its many nations were in intimate and generally friendly contact. Britain and Egypt, Syria and Lusitania, Greece and North Africa were all within the same *Kulturkreis*, and the two international languages, Latin and Greek, were read and understood by most of the scholars and philosophers.

For most purposes the numerous vernaculars had been displaced by one or the other international language, except in the east where Aramaic—itself a learned language—was spoken over a wide area. But nowhere was there any serious linguistic barrier. Numerous bilingual and even trilingual scholars made it easy for ideas to pass from one national group to another. Yet, during these halcyon days, science deteriorated and superstition grew. A number of mystery religions arose and flourished, feeding upon the increasing credulity and superstition of the public at large.

Many causes have been given for this intellectual retreat. The usual procedure is for a historian to pick out what he likes least about the classical world and assign to it the responsibility for the collapse of intellectual standards. Historians of biology have tended to react like their fellow historians and, consequently, they tell us little about any real cause for the decline of science. Thus, Singer ascribes the deterioration in part to the loss of independence of the Greek states and in part to the severely practical nature of the

Romans, who knew no better than to concentrate their attention on the applied sciences (*History of Biology*, pp. 63–67). Classical science reached its peak, however, after the several states of Greece had lost their independence, and the Romans themselves were never more than a minority in the whole empire.

Nordenskiöld recognized that the classical world possessed a favorable physical climate for the advancement of science, but he held that its spiritual climate was unfavorable. He assigned its loss of scientific standards to a prevailing conception of life that arose as a consequence of the subjection of the civilized world to the Roman Empire (*History of Biology*, chap. 8).

But any connection between these assigned causes and their assumed effects is obscure, and every hypothetical explanation is tainted with the *post hoc* fallacy. Nor can another favorite explanation—the intolerance of the Christians to heretical speculations—be offered, because the Christians were a minor and not too influential sect until the fourth century after Christ, and by that time the intellectual decay was almost complete. Before the fourth century—before the Christians destroyed their opponents—the citizens of the Empire had far more personal liberty, and suffered much less from religious intolerance and persecution than did the people of Europe during the Renaissance—during the period when science again was flourishing. Science was actually reborn in a period when wars were fought with unprecedented ferocity, and when heretics could be and often were burned at the stake.

The intellectual decline in the classical world may have been the cause rather than the effect of the events that destroyed its civilization. We can give any number of biological explanations for the steadily falling intellectual level—explanations that fit the known facts—but we have no way of evaluating any of the biological causes quantitatively, nor do we have any way of estimating their relative importance. Until we have more basic knowledge, speculation is futile.

The Roman Empire, as we stated, split in two; the western half spoke Latin, the eastern half Greek. These two linguistic divisions had always existed, but the two languages were no obstacle to communication so long as the learned classes spoke both. We know that there was a constant flow of Greek manuscripts into Italy. The political split, however, did emphasize the language difference and did slow up communication. The Greek manuscripts in the west

slowly disappeared, the unfamiliar Grecian letters were washed off, and the valuable parchment on which Greek was written was frequently used for a second time. The Latin written on these palimpsests was often trivial, but at least it could be read and understood by the Latins.

The political split of the Empire was accompanied by a religious schism. The Greek and Roman churches separated into two mutually hostile factions. The two parts of the Roman Empire now had each its own language, its own religion, and its own government. Intellectual contact between the two halves came almost to an end, although some commercial contacts remained. How completely the intellectual communication was interrupted is shown by the fact that, centuries later when the Latin world was trying to recover some of the Greek learning, it had to get what it could through the Arabs, even though the commercial channels between Constantinople and the Italian cities of Venice and Genoa were open. Apparently, few classical manuscripts made the trip in the merchant ships.

In the seventh century, the Arabs, speaking a language closely related to Aramaic, erupted into Syria, Palestine, and Egypt. In these countries they submerged the Greek language and culture. Inspired by the new religion of Mohammed, the Arabs swept the Latins out of North Africa, and even took control of Spain. The Roman Imperium was now hopelessly fragmented into regions, each of which had its own language, its own political organization, and its own religion. Contacts between the segments of what had once been a single civilization, were rarely friendly. Generally they were military contacts, and almost never were they scholarly. New ideas and even factual knowledge passed the new frontiers slowly and with many impediments.

The Latin West was overrun in part by Germanic barbarians, but Latin was preserved by the Church, where it remained the language of religion and of the little learning that remained. Centuries later, when the universities were established, it served as the language of the secular scholars. The common people, however, spoke one or another of the vernaculars that had arisen during the years of political chaos. Greek continued to be the language of the Byzantine Empire, but the area in which it was spoken contracted every time the Empire lost a province. On the other hand, Arabic extended its

sway and became a truly international language, one spoken all the way from Spain to India. The southern portion of what had been the Roman Empire became merely the western part of Islam.

Meanwhile, far to the east—in China—another civilization had arisen, a civilization that had been influenced in part by the older cultures of the Middle East. But now, during the period when the Mediterranean world was stagnating, Chinese civilization spurted ahead. The diffusion of information was now reversed. New technical inventions flowed from China to the West. At no time was the stream a torrent—it was never more than a trickle—but the effects of its flow were cumulative. During the Middle Ages, the Chinese invented gun powder, which they used in firecrackers to frighten their enemies, and the mariner's compass (but the Chinese compass pointed south!). The Chinese also learned to make paper and to print from movable type. When the Europeans acquired these arts, the way was paved for the Renaissance. One of the greatest of all medieval inventions, however—the "Arabic numerals"—came from India.

What we know of the biology that persisted through the medieval period is found mainly in a number of incidental passages included in the writings of the poets, philosophers, physicians, and encyclopedists, and this was the case in all the separate cultures. Whatever unity science may have had in the past, it now lost. Many of its scattered fragments persisted, however, both in the lands that had been governed by Rome and in the nations farther east. Wherever any fragments of science had been preserved, some few scientific advances were made. The Arabs, Persians, Indians, and Chinese all added their bit, and the sum total of scientific knowledge increased, even though individual scientists were isolated in different cultures and labored in ignorance of each other's existence.

From the seventh century until the thirteenth, biological knowledge remained fragmentated, and the fragments were imbedded in very different cultures. It is true that each of the cultures developed its own characteristic biology, but the biologies did not differ from each other as much as we might suppose. Biology, like all of the sciences, has a certain amount of autonomy and, to some extent at least, it can keep itself free from the worst of the cultural contaminants. As we know, scientific errors will often stem from the better thought of notions that are current at a given time and in a given

place, but a scientific discovery can come only from the material of science itself. The facts that are discovered do not depend upon any of the fashionable convictions. This can be illustrated with an example: although the circulation of the blood is the same in all cultures, a particular culture may determine only whether or not the circulation is discovered—not how the blood circulates.

Beginning with the eighth century, the Arabs dominated biology as they did all the sciences. Their contributions were many times as numerous and much more important than those of the Latins, Byzantines, and Chinese. While the Latins, perhaps, ranked second —a very poor second—the Byzantines accomplished almost nothing. Chinese biology was in an early stage of its development and some time would elapse before it would make its own great and very practical contributions. For the next five centuries biology would be essentially an Arabic science.

The Germanic invasion of Britain, Gaul, and Italy, together with the Muslim conquest of Spain, had reduced the Latin half of the Roman Empire to political impotence. Before the Latin West could recover, the Germanic tribes would have to be tamed and amalgamated into the indigenous peoples they had conquered. A new and stable population base would have to be created before Europe could again contribute to the arts and sciences of civilization.

Strangely enough, the first biological contributions to come out of the new Europe were from France and England—from the regions where the barbarians had settled. These contributions were not major—their scope was strictly limited—but they did show some originality. They dealt with medicinal herbs, and their originality lay in the fact that they included descriptions of some of the herbs that were native to northern Europe—herbs that had not been described by the classical botanists.

The first of these northern Europeans to write on the plants he observed was Walahfrid Strabo (808–849), who was born in Swabia. He acquired the epithet "strabo" by being cross-eyed and, probably, also by squinting. As he had intellectual and scholarly interests, he entered a monastery, where at the age of fifteen, he began to write poetry. He was a versatile teacher and scholar, and he did many different things during his short life. But his chief interest was in writing poetry. He even aspired to be the Virgil of the Carolingian Age. In 829 or 830, he was called to the court of Louis the Pious,

the son and heir of Charlemagne. There he served as the tutor of Louis' son Charles, the son who grew up to be known as Charles the Bald, and who ruled a third of the empire that Charlemagne had assembled. Walahfrid ended his days as abbot of Reichenau.

We are concerned here only with Walahfrid's botanical work *De Cultura Hortorum*, a poem in 444 hexameters, describing twenty-three herbs that grew in the monastery garden. Walahfrid emphasized the medicinal properties of the herbs. However, we get the impression from his verses that this was not his main interest but only his excuse for writing poetry. He liked the plants for their own sake; he described them not only poetically and with feeling but also accurately. He obviously relied on living specimens and he included in his poem some herbs that were native to northern Europe. Thus his work was not merely a repetition of classical herb lore written in Latin verse. None of his medicinal herbs was very potent and none could be classified as "strong medicine." Walahfrid was merely a poet who loved plants.

A century elapsed between the time of Walahfrid and that of the second contribution from northern Europe. The *Leech Book of Bald* (Cockayne, 1864) dates from the first half of the tenth century. Little is known about Bald, under whose direction the book seems to have been compiled, except that he was probably in touch with the great King Alfred, who had worked so hard to civilize the English. This *Leech Book* represents almost the apex of Anglo-Saxon plant lore and is the oldest leech book in any vernacular. It gives us an excellent picture of the medicinal plants that were used in northern Europe. The Anglo-Saxons knew and used approximately 500 plants and, at the time, their knowledge of plants was probably superior to that of the southern Europeans.

Still another century passed before there was a third contribution. During the second half of the eleventh century the *Macer Floridus Viribus Herbarum* appeared. This was a poem in 2,269 hexameters describing the medicinal virtues of seventy-seven herbs. It is generally conceded that it was written by one Odo, who came from Meung on the Loire river. It was an immensely popular poem, as is shown by the number of manuscripts in existence and by the fact that it was translated into French (twice), German (twice), Danish, English, and Hebrew. According to Frisk there are eight English manuscripts that are authentic translations and several that

claimed to be because of the *Macer's* great prestige. Frisk has recently edited a Middle English translation that is now in the Royal Library of Stockholm.

It is important that we get a glimpse of the *Macer* because it illustrates not only the classical traditions of medicinal herbs, but also the herbal tradition that gave rise to the great herbals of the sixteenth century—the herbals that were so important in the development of modern botany. The real value of these herbals was not their reputed value. Supposedly they were guides to the medicinal properties of plants, but in reality the pharmaceutical value of most of the herbs was entirely imaginary. The herbs were placebos for both physician and patient. But even so, our medieval and Renaissance predecessors were not far behind us. Even late in the nineteenth century, a great many of the plant drugs that were in the official pharmacopoeia were worthless. Here we will quote a single example from the *Macer*, an account of the medicinal properties of an herb that we all know. We use it today in our juleps and to flavor our iced tea. It is mint (*Mentha crispa*). Needless to say, mint does *not* affect us as the *Macer* said it should. (From Frisk's edition of a Middle English manuscript, p. 127):

> Mynte is hoote and drye in the II: de gre. –Vis prima. For the stomak and wormes. Mynte drunken helpith the digestion, comforteth the stomak and staunceth vomite and sleeth the wormes in the bely. –II Warshe thi ballokes [testes] with the water tha mynte is soden in, and that wole hele many grevaunces of him. –III For the voyes. Mynte iuus [juice] made leuke and drunken wole clarifie wondrely the voyes. –IV for tetes [teats]. Mynte stampid and leyde to the tetes makith it ful of mylk. –V For the ere. Held in the ere the iuus of mynte with hony, so shalt thou abate the ache of the ere. –VI For the tunge. Frote thou the tunge ofte with myntes, and hit wole hele the roughnesse of the tunge. Mynte iuus drunken with sapa wole delivere sone [son] a woman and aleye here throwes. Sapa is whit and smal wyn or het wyn medled with the iuus of some herbe. –VII for houndesbyting. Mynte stampid with salt and leyde up-on wole hele byting of an hounde. –VIII For hem that speten blode. The iuus of myntes medled with vynegre helpith hem that speten or spewen blode. –X Mynte iuus underput to the matrices a-fore the cunte maketh that the woman shall not conceyve at that tyme. –XI Mynte iuus drieth bocches [botches-ulcers]. –XII Mynte stampid and leyde to wole help the woundes of the hede. –XIII Mynte iuus medled with rynnynges [rennet] wole not suffre chese for to root that is made with that rynnynges. Ley the grene mynte above the chese, and it wole doo the same.

We need do little more than mention the Englishman, Alexander Neckam (1157–1217), whose mother was the wet nurse of Richard Coeur de Lion. He composed *De Naturis Rerum*, a popular encyclo-

pedia that included a description of the compass. He also described the barnacle-goose—the goose that was hatched from the barnacles that fell from the barnacle tree into the water. Incidentally, John Gerard in 1597 wrote in his *Herbal* that he had personally seen these geese.

Compared with the Latins, the Arabs were highly civilized—worthy inheritors of the Greco-Roman traditions. Their scholars were more sophisticated than those of northern and western Europe. They had broader interests and were better educated. In spite of the fact that Islam extended from Spain to India, it remained unified. The pilgrimages to Mecca kept the lines of communication open, and the Muslim scholars found it easy to travel from one center of learning to another. And there were many such centers: in Syria, Egypt, Mesopotamia, and even Spain. The language of the people, unlike the vernaculars of the Europeans, was also the language of the learned. Thus the Islamic scholars and philosophers did not have to master a second language before they could begin their studies. For some five hundred years, Arabic was the language of the most civilized fraction of mankind.

We shall begin our account of Arabic biology with the work of an eighth century Arab, Al-Asma'i. He was born in Basra in either 739 or 740 and died there about 831. The fact that he was an Arab is mentioned here because a great many of the earlier scholars of Islam were Greeks, Syrians, and Persians—cultured and learned men—who had been converted rather drastically to the new faith. It was these converts who brought the classical learning of the Greeks and Romans to the Arabs. The Arabs themselves made their contributions later.

We should not overlook the fact, however, that the Muslims did not always compel those they had conquered to accept the religion of their conquerors. During their periods of tolerance—and there were many such periods—the Mohammedans allowed both the Jews and the Christians to remain Jews and Christians. Islam even gave a refuge to the Nestorians who had fled from the persecutions of their fellow Christians—the Christians who remained orthodox.

Al-Asma'i was a very learned man and he wrote voluminously. In fact he was such a famous scholar that the "good" Harun al-Rashid called him to Bagdad to be the tutor of his son. Al-Asma'i composed works on the horse, on the camel, on the sheep, and on wild animals. In fact, he helped preserve and spread zoological knowledge

throughout the Arabic world. His work on the making of man shows that the Arabs at this time had a considerable knowledge of human anatomy. Al-Asma'i was a transmitter of science rather than an original investigator.

A contemporary of Al-Asma'i, who also lived at Basra, was Al-Jahiz (d. *ca.* 868–69), the goggle-eyed, whose life also overlapped that of the Latin, the crossed-eyed Walahfrid. Al-Jahiz was a scholar, who wrote many books on many subjects. His chief interests were literary and theological rather than scientific but, as he was a scholar of his time, he wrote on practically every subject that came to his attention. His biological works covered considerable ground, but he gave a little extra emphasis to animal hybridization and animal psychology. He wrote a *Book of Mongrels*, a *Book of Blacks and Whites* (his grandfather was a Negro slave who had been freed), and a *Book of Mules*. His *Book of Women* and his *Book of the Wheat and the Palm* complete his biological works except for his *Book of Animals*, which, from our viewpoint, is by far the most important.

The *Book of Animals* shows some Grecian influence, but it is primarily an Arabian work. In fact, it is the first product of nature study among the Arabs. In it, the unity of nature is emphasized, as is the value of all parts of nature for scholarly investigation. Al-Jahiz is even credited with having evolutionary views, derived in part from his observation of the adaptation of animals to their ecology and in part from his studies of animal psychology.

Al-Jahiz was greatly impressed by the unity of nature. He believed in spontaneous generation, a doctrine which seemed reasonable to him because he believed that the earth contained within itself a fusion of both the male and female elements. He thought that animals had been derived from certain forms of plants, but that they also possessed traces of human characteristics. And he also held, conversely, that human beings possessed animal traits. The chasm that separated man from the animals was, he thought, bridged by intermediate races. Sexual aberrations between different species of animals and between man and animals undoubtedly took place, but they were, he believed, entirely unsuitable. By stretching the term slightly, we may even call Al-Jahiz an evolutionist.

The biological interests of Al-Jahiz were primarily zoological, but Muslim botany was also thriving. Unfortunately, the outstanding treatise of the time has been lost. It was written by Al-Dinawari

(815–895) a Persian historian, lexicographer, astronomer, and botanist (Sarton, 1927). His chief works were (1) a history, labeled appropriately enough a *Book of Long Stories*, and (2) a *Book of Plants*. The *Book of Plants*, as we have indicated, is lost but many extracts from it, including some 300 to 400 descriptions of plants, are quoted by later writers. Al-Dinawari's general and theoretical statements concerning botany and agriculture show that he was an excellent scientist, while the details of his work show that he was careful and accurate. For example, he gave precise and workable directions for hand-pollinating the date palm. The existing fragments of his *Book of Plants*, if collected and edited, should add considerably to our knowledge of ninth century botany.

Contemporary with the Anglo-Saxon *Leech Book of Bald* was the work of Ibn-Wahshiya (Sarton, 1927), who flourished before 912 and whose Nabataean forgery we have already discussed. He was born in Iraq of a Nabataean family. Thus he belonged to the ancient Babylonian stock and not to the dominant Arabians. He tried to show how much the elder civilization had accomplished and how much the ancient Babylonians had known. While he could not read the cuneiform tables, and his reputed translation thus was his own composition, he did include in his forgery a great deal of ancient lore. A great many of the later Arabic writers took him at face value.

One of the more important Muslim scientists of the tenth century was Al-Mas'udi, who was born in Bagdad before 912 and died in Cairo in about 957. He was a historian, a traveler, and a geographer. The most important of his works is his famous *Meadows of Gold and Mines of Precious Stones*. This is an encyclopedia remarkably complete and rich in historical and geographical information. Its biology is scattered and in no way remarkable but the work is useful in showing the biological beliefs current in Islam and the general status of the science in the Arabic world. Thus in describing Egypt he wrote, "It is a mine of gold, of precious stones, of emeralds and of all sorts of riches, a soil rich in harvests and in male palms."

We may use an excerpt from Al-Mas'udi to illustrate the Islamic explanation of the origin of the human species and of the differences that exist among the several races of mankind. The Koran had stated (15:3); "We created man of dried clay of black mud formed into shape." The creation of man and the origin of the

differences between the races is accounted for by Al-Mas'udi as follows: God sent the angel Gabriel to earth to fetch him the clay for Adam's body, but earth refused to give him any. Then he sent the angel Michael who was refused in turn. Finally God sent the angel of death and the earth yielded the clay. "And he took black, red and white earth: for this reason the sons of Adam are of different colors." This is essentially the account of our human origin that was given earlier by Al-Tabari (838–923) and later by Al-Athir (1160–1233).

Al-Mas'udi's concept of the biological effects of inbreeding and cross-breeding seem somewhat bizarre. Twins, he thought, were more closely related than were ordinary brother and sister. At this time there was no general agreement as to the desirability or undesirability of brother-sister marriage. He wrote, as translated by Sprenger (pp. 62–63):

> Those who believe on the Old Testament say, Adam married the twin sister of Habil to Cain, and the twin sister of Cain to Habil, so that the twins would be separated in marriage. The law of marriage adopted by Adam, was, therefore, to separate, as much as possible, persons allied by relationships, in order to prevent, by separating them, the bad consequences, and weakening influence upon the offspring; the Mágians are of opinion that Adam did not object to the marriage of relatives; hence, they are not against it. They have some mystery respecting this, according to which they think it good that a man should marry his sister, and the mother her son.

When we come to the eleventh century, we find that the Arabs still dominated the sciences almost completely. In the first half of the century, two of the greatest scientists of all time appeared in the Arabic world. Both came from the eastern portion. Al-Biruni (973–1048) was born in Kiva and died in Afghanistan, while Ibn-Sina (980–1037) was born near Bukhara and died in Hamadhan; both wrote in Arabic and in Persian. Al-Biruni was an encyclopedist, a traveler, a geographer, a philosopher, a mathematician, and an astronomer. He was a biologist only incidentally. Sarton (1927) reported that he described such monsters as Siamese twins. He also recorded certain facts about flowers—e.g., that they have 3, 4, 5, 6, or 18 petals and never 7 or 9.

With the possible exception of Aristotle, Ibn-Sina—known to the Latin world as Avicenna—was perhaps the most influential philosopher and scientist of all time. His philosophical encyclopedia and his writings on medicine and drugs were known and respected in Christendom as well as in Islam. Indeed his *Canon*

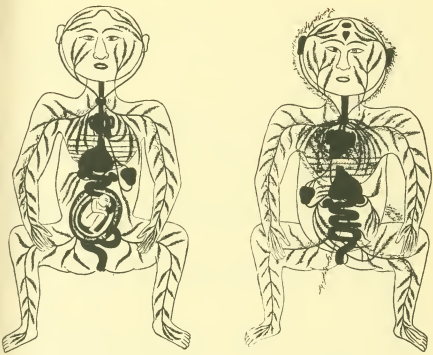


Figure 2. Arterial diagram of man and woman, from Persian MS No. 2296, India Office, London. (From Choulant, 1920.)

of Medicine, together with the works of Galen, remained the great authority until the Renaissance. Ibn-Sina was also a mathematician and an astronomer. His voluminous writings on medicinal plants were by far the best of his period. In general his biology was as good as any that existed in the eleventh century, and his writings did much to re-introduce the zoology of Aristotle into western and northern Europe. The numerous and exceptionally capable Latin scholars who appeared two centuries later were all indebted to Avicenna.

An adequate presentation of Ibn-Sina's biology would involve a description of all of the biological knowledge of the time. Here we can point out only a few of his ideas and emphasize some that may seem rather odd to us, admitting as we do so that this procedure is not completely fair. Like Aristotle, Ibn-Sina was a teleologist. He took many of his concepts and his basic assumptions from the

Greeks—e.g., his anatomy is the anatomy of Galen. He followed the Koran in assuming the existence of a basic sexuality in all of nature—a concept that may have persisted in the Near East for some two thousand years—at least since the time the Phoenician *Baal* was one of the more important deities. To quote from the Koran (Surah 36:34–35):

> Glory be to Him who created all the sexual pairs, of that which the earth groweth, and themselves, and of that which they know not.

Ibn-Sina wrote, as cited by Gandz (1935), that romantic love (*al-ishq*)

> is not peculiar to the human species, but pervades all existing things of the celestial and of the elemental and the vegetable and the mineral and the animal, and that its meaning is not perceived nor known, and is rendered more obscure by explanation thereof.

In explaining how human semen acquired its ability to produce another generation, he described a form of pangenesis that resembled Buffon's. From his *Canon of Medicine* (Bk. III; Fen. 19: Tract 1: Cap. 3):

> Truly, it seems to me that it is not necessary that the sperm come from the brain only, although the fermentation of the sperm finds its source in the brain. And what Hippocrates said concerning the existence of the two veins is true: but it is necessary that the source be from every principal member of the body, and that there be from the other members a refund, even to the very origins.

By the time the twelfth century arrived, the intellectual decline that the whole world had experienced had come at last to an end. For a short while, the civilization that remained seemed to stabilize; then came recovery. The improvement, however, was slow at first. Science began its advance but it had to start at a very low point. From this time on, however, science continued to progress. Its growth was never interrupted seriously. Always, somewhere in the world, new discoveries were being made, new theories formulated, and new insights obtained. Some time was to elapse before science would be pursued for its own sake—its progress, seemingly, was only incidental to the recognized usefulness of new knowledge in practical and applied fields—but when, at last, knowledge was sought for the sake of knowledge and because of the dislike of ignorance, science began to assume its modern aspects.

There was little biology in the twelfth century that was not applied. Botany was a by-product of pharmacology, zoology of

falconry and veterinary medicine. The search for medicinal herbs, however, resulted in the identification of herbs in general, and attempts to describe the herbs led to the development of botanical iconography. The illustrations of plants became less conventional and more realistic. The Pseudo-Apuleius manuscripts, copied during this century, showed pictures of plants that were drawn from nature. A contemporary English manuscript from Suffolk furnished the prototypes of the first printed illustrations of plants.

Falconry was becoming a popular sport. In fact it became the chief sport of the European aristocracy—that is, next to wenching and drinking. Falconry, the art of hunting with hawks, may have originated in Egypt or Babylonia but, if so, it diffused into central Asia at a very early date. In Asia it reached its full development, and from Asia it spread back again into the Arabic world, and from the Arabic world into the Latin West.

During the twelfth century, biology grew erratically, intermittently, and at very different rates in the Arabic, Latin, Byzantine, and Chinese cultures. Arabic culture was still the most advanced but, while Arabic science was still better than any other, it was beginning to show signs of its coming stagnation. The Latin world was improving rapidly, more rapidly than the direct evidence would indicate because at this time it must have been gaining the momentum that would lead to the almost explosive progress it made during the next century. The Greek or Byzantine scientists could best be described as sleeping peacefully, preserving their manuscripts and records but doing nothing of importance. On the other hand, the Chinese were beginning to stir and had already started the work that would put them in the vanguard of civilization within the next two hundred years.

The great twelfth century Arabian philosopher, Ibn-Rushd—known to the Latins as Averroës (1126–1198)—was born in Cordova, in the western marches of Islam. He wrote commentaries on the work of Aristotle, but unfortunately his comments on the zoological portion are lost. He also wrote a medical encyclopedia, based in large part on Galen, an encyclopedia that contained treatises on anatomy, physiology, pathology, hygiene, and materia medica. In addition to Galen he used more recent and, perhaps, even contemporary sources of information. He recorded, for example, that no one could have smallpox twice. His support of Aristotle challenged many of the prevailing views, and he became

the center of an almost violent controversy in both the Arabic and Latin countries. He did much—perhaps more than any other single individual—to rehabilitate Aristotle.

At the other extremity of Islam, Nizami-i-Arudi, a Persian, wrote *Four Discourses* sometime between 1150 and 1160. This work contains some incidental biological information. It was, however, merely the information that was current in Persia. He seems to have had an extremely vague notion of evolution, because he tried to connect the mineral, vegetable, and animal kingdoms by means of transitional forms. The following is quoted from *Jour. Roy. Asiatic Soc.* 1899, pp. 626–627:

> That these kingdoms should be connected successively and continuously, so that in the mineral kingdom the first thing which attained completeness and underwent the process of evolution became higher in organization until it grew to coral, which is the ultimate term of the mineral world, until it was connected with the first stage of plant life. And the first thing in the vegetable kingdom is the thorn and the last is the date palm since it needs the male to fertilize it so that it may bear fruit.

Also from the eastern part of Islam came Abd-al-Latif (1162–1231), who was born, and lived, in Baghdad. He was a philosopher, a scientist, a physician, and a voluminous writer. He traveled widely and described in detail the animals and plants he saw—particularly those of Egypt. He recorded the fact that, where many varieties of citrus fruits were growing near each other, the spontaneous seedlings that sprouted in the neighborhood showed great variability, and that the fruit of these seedlings was of many kinds. He seems to have had some notion of plant hybridization, but hardly an accurate one, because he told how the banana could be grown from a date seed that had been implanted in the corm of the Colocasia. He also told how the Egyptians hatched eggs in incubators, in which the heat came from the fermentation of manure. He is best known, perhaps, for his description of the anatomy of the skeleton.

Ibn-al-'Awwam (who flourished during the second half of the twelfth century), like Averroës was born in Spain. His treatise on agriculture, written late in the century, is undoubtedly the most important of all medieval treatises on the subject. It was based on Greek and Arabic originals, and also on personal, though not critical, observations. He relied especially on the Nabataean agriculture and dealt with some 585 plants in all, including more than 50 fruit

trees. He recognized the existence of sex in the date palm and assumed tacitly that practically all trees reproduced sexually, but his identification of the male and female trees was not based on flower structure. Cultivated varieties he assumed routinely were the females, while the wild trees were males. He even stated that the terebinth, or turpentine, tree is the male of the pistachio! However, Ibn-al-'Awwam did identify the role of the cynips insects in caprification and he stated that their presence was necessary for the production of good figs. He also paid especial attention to the sympathy and antipathy of plants and told which plants could be grown near each other without injury. And he told how certain plants altered the taste of others that grew nearby.

On leaving the Arabic world for that of the Byzantines, we are struck at once by the latter's intellectual sterility. From the time of the agricultural compilation of Cassianos Bassos in the fifth century, no biological contribution worthy of our attention came from Byzantium until the eleventh century, when Michael Psellos (1018–1078) wrote a medical text. Psellos was a Neoplatonic philosopher who flourished at Constantinople. He was also a historian and a mathematician. He wrote a medical text in verse, a medical glossary, and a text on medicinal bathing. According to Sarton, he was at his best reminiscent of Voltaire, but in general, he was merely one of the few pre-Renaissance Platonists. His statements on biological subjects are scattered throughout his medical works. Again, the samples that we cite, samples which fit him into his age and location, may not be completely fair. He held, for example, in *De Omnifaria Doctrina* (chap. 83) that both the male and female contributed semen to form the embryo, but that the male semen was the stronger and developed into the stronger parts of the body—i.e., the bones, sinews, nerves, and veins, while the female semen produced the blood, the bile, and other weaker parts.

He also held that roses, lilies, and all fragrant plants were given a better odor if they were grown near plants that had an evil odor like garlic; but that a bitter plant when grown under a fig tree becomes more bitter. He also remembered caprification but only vaguely, as shown when he stated that fig trees produced more pleasant figs when they were grown near wild figs. This, he held, was because whatever natural bitterness there was in the figs passed on into the wild figs. Psellos, however, did help to pass the classical knowledge of biology on to the time when learning was revived.

The state of Byzantine science in the twelfth century is well illustrated by the historical work of Michael Glycas, who wrote his history between 1143 and 1156. His work started with the creation of the world and ended with the events of his own time. His account of the creation of animals and plants contained much incidental biology, but it was a biology that was based on the *Physiologus* and on the writings of Aelian. It was moralistic and contained the usual mixture of authentic information and superstition.

His work abounds in biological oddities, such as his description of the female vultures undertaking a campaign for impregnation because of their fear of barrenness. This involved certain difficulties because, he believed, there were no male vultures. The females were normally impregnated by the south wind but if no south wind presented itself to them they spread their wings toward the east wind. Thence they were filled with the wind blowing in through their open mouths and they bring forth animals, not wind eggs. On the other hand, he describes accurately how the torpedo shocks fishermen without actually touching them and, in his discussion of action at a distance, he cited correctly the fact that a magnet attracts iron even though air intervenes. He also noted that the male date palm can impregnate the female from a distance but his account of the act shows what we may call the influence of his creative imagination (*Annales*, Pt. I, V. 28, p. 37):

> They say that in palms some are certainly males and others females, and what is something new and unheard of, a natural love exists among them. The female lowers itself to the male, as if excited with some passion and seeking an embrace. Then, as though sensing that the desired fruition had been attained, it raises up its branches again and returns to its earlier position.

It is perhaps too early for us to include an adequate account of early Chinese biology in any general history of the subject. We know that the Chinese were active and that their works on agricultural and medicinal botany deserve all respect. This is about the extent of our knowledge. As early as the third century, a Chinese botanist, Chi Han (fl. 290–307), compiled a flora of southern China. In the fifth century, Hung-Ching (451–536) wrote a *materia medica,* which was revived and added to by Ying Kung late in the seventh. Indeed the Chinese were continually revising their works on medicinal plants and bringing them up to date. They were also leaving detailed records of their more valuable

cultivated plants, one of the earliest of which was written by Lu Yü.

Lu Yü (Sarton, 1927) was a contemporary of the Arabian, Al-Asma'i. He became a hermit in 775 and died in 804. As he was Chinese it seems appropriate that he wrote a book on tea—in fact the earliest book ever written on tea. His work was a definitive treatise which covered the ground thoroughly. He described the botany and the origin of the plant, the utensils for gathering and curing the leaves, and the implements for making tea. He told of tea drinking, of its history, and of the districts where tea was grown.

Other Chinese food plants seemed to call for detailed and systematic treatment. Tang-ning, who flourished in 988, wrote a book on bamboo sprouts, and in 1059, Ts'ai Hsiang wrote a treatise on the litchi (*Litchi sinensis*), which is the earliest of all monographs on a fruit tree. Ch'en-fu (1149) wrote an extensive work on husbandry, and included in it an account of sericulture.

Han Ch'an-Chi, the governor of Wen-Chou in 1178, was the agriculturist who wrote a book on oranges, our earliest monograph on the citrus fruits. He described twenty-seven varieties, but from his descriptions it is obvious that he covered more than the varieties of *Citrus sinensis*. He described the sour orange, the kumquat, the tangerine, the pumelo, the citron, the shaddock, and perhaps the lemon and the lime. He described his "oranges" as yellow, golden, red, and green; sweet, sour, and bitter. Some were edible only after the first frost. He gave detailed directions for cultivating the citrus fruits, for transplanting the trees, and for gathering and storing the fruits. He told how the orange plantations were best irrigated and he listed the medicinal properties of the citrus fruits. At this time the Chinese, obviously, were sophisticated farmers and had a scientific approach to horticulture. They would soon accomplish a great deal more.

6

Subhi Kazib: The False Dawn

The thirteenth century might be considered the false dawn of the European Renaissance. Light appeared, but the startling brilliance of the true Renaissance was still two hundred years in the future. By the thirteenth century, however, the learning of the classical world had reached western Europe by way of the Arabic translations and, while these were inferior to the Greek originals, they were accompanied in their migrations by some of the works that the Arabs themselves had written. Europe actually acquired more knowledge in the thirteenth century than it had lost when the classical world collapsed.

On the whole, the Europeans received the ancient learning hospitably, and they fitted it into their philosophical and religious beliefs whenever they could. Aristotle was rehabilitated, Plato lost his philosophical monopoly, and a number of questions that had been taken as settled were re-opened. The stagnation that had afflicted Western learning was definitely on the way out. But most important of all, Europe produced a number of very able men.

Meanwhile, the Arabic world did little more than mark time; thus it fell behind the Latin. The Greeks slumbered on but, thousands of miles to the east, the Chinese were beginning a renaissance of their own. In the next century they would lead the world in the arts and sciences of civilization. Soon their biology would be far in advance of the biology of the other centers of civilization.

The thirteenth century, however, was a Latin century. The important books were written in Latin, and the Latin philosophers,

theologians, and encyclopedists were twice as numerous as their counterparts in the Arabic and Chinese worlds. Their writings, moreover, were all-inclusive and unbelievably voluminous, so voluminous, in fact, that we can present them only as a few selected samples. It is not difficult, however, to show the extent of the current biological knowledge, and the attitude of the Latin scholars toward what biology they knew.

We can introduce the biology of this period most effectively by examining the work of one of its greatest men, Frederick of Hohenstaufen (1194–1250) who, for the last thirty years of his life, was Frederick II, Emperor of the Holy Roman Empire. Frederick was a true patron of learning; he surrounded himself with the greatest scholars of his age. Some of these learned men were Christians, but most were either Jews or Muslims. Indeed the scholars and philosophers of the world found a welcome at his court. He established the University of Naples in 1224, and in spite of his many political and administrative duties, he completed personally one of the best works on zoology in the Middle Ages, his *De Arte Venandi cum Avibus* ("Art of Falconry").

Frederick's book on falconry is detailed and elaborate. In addition to a description of the technique of hunting, it contains not only the older information which he took from Aristotle and the Arabs, but also the anatomical observations on birds that he made from a study of actual specimens. Much of Frederick's knowledge came from his own careful experimentation, and much of it was new. For example, he reported that birds had hollow bones. He described the structure of their rump glands, their lungs, and the muscular attachment to their keeled breast bones. He also studied the mechanics of their wings in flight. His zoological interests extended well beyond birds, and he assembled a menagerie that contained elephants, camels, dromedaries, lions, leopards, monkeys, and even a giraffe in perhaps the first menagerie ever seen in Europe. With Frederick, the intellectual ferment was well under way.

Frederick was not alone. For the first time in centuries, Western Europe had a number of excellent scholars working at one and the same time. Europe was an intellectual unit. Her scholars all wrote and spoke the same language, and they were all connected with the same religious organization. They traveled widely, visited one another, and gave and received the stimulation that only scholarly companionship can give.

They were true contemporaries, born near the beginning of the century; and those who were octogenarians (a remarkably high percentage), died not far apart. Unfortunately we do not know the exact birth and death dates of some, and these we can date only by noting the years in which they flourished or the period in which they published. It is worth recording that the outstanding scholars in Islam and in China were almost exactly their contemporaries.

The following list of the men who wrote on biological topics and who helped to make the thirteenth century such an exciting time shows graphically how their lives coincided:

Bartholomew the Englishman (fl. 1220–1240)
St. Albert the Great (*ca.* 1200–1280)
Thomas of Cantimpré (*ca.* 1204–1280)
Roger Bacon (1214–1292)
Vincent of Beauvais (–*ca.* 1264)
St. Thomas Aquinas (1225–1274)

The Arabic scholars were:

Al-Qazwini (1203–1283)
Ibn-an-Nafis (1208–1288)

The Chinese, Ch'en Ching-i, completed his botanical encyclopedia in 1256.

No single generation in the Classical World produced a group of scholars comparable to the above.

Bartholomew the Englishman (*Bartholomaeus Anglicus*) did not add to the biological knowledge of his time, but he spread it far and wide in his extremely popular encyclopedia, *De Proprietatibus Rerum*, written between 1230 and 1240. This work was translated into Italian (1309), French (1332), Provençal (before 1391), English (1397 or 1398), and Spanish (in the fifteenth century). The English translation, *All the Proprytees of Thynges*, by John of Trivisa, was printed at Westminster in 1495. For almost three hundred years, *De Proprietatibus Rerum* was an almost universal textbook for university students, who could rent copies of it for study even if the purchase of a copy were beyond their means. Its chief purpose was the teaching of theology, but it also covered natural history, and in doing so it spread both information and misinformation. Its nineteen books dealt with the properties of all

things. Book 3 was on psychology, books 4 and 5 on physiology, book 7 on medicine, book 12 on birds, book 13 on fishes, dolphins, and whales, book 17 on trees and herbs, and book 18 on animals.

Bartholomew's treatment of plants is perhaps his best. He knew the local herbs from personal experience but he completely missed the significance of pollination. From the first printed edition in English:

> And he settyth double hinge of palmes. Male and female. An ye male blowyth furste: and after the female burgenyth and blowyth. And the female bereth not fruyte: but if she be so nyghe the male: that the smell of ye male maye come wyth the wynde to the female. . . .
>
> And the female growyth not well nother beryth fruyt wyth the male. And off the male be felde: theynne is the female bareyne after two dayes out. Yf leves & floures of the male be put abowthe the rotes of the female: thenne by comforte of the male as it were by comforte of ye werke of generacion the female takyth afte vertue and strength.

Bartholomew, and presumably those who read his book, believed many weird things about the animals that lived in the far parts of the world. These foreign animals were perverse and sinful and, because of their lack of standards, they hybridized promiscuously:

> The perde is lecherous and gendreth with the lyennesse: Of ye bastarde generacion comyth leoperdus.

Bartholomew states further:

> And that he is gendryd of the pard lackyth that nobylyte/ The lyon knowyth by smelle yf the pard gendryth with the lyennesse: and ariseth ayenst the lyenesse that brekyth spouseheds: & punyssheth her full sore but yf she warshe her in a ryver & thenne it is not known to the lyon.
>
> Hiena is a cruel breast luke to ye Wulfe in devouryng & gloteny and resith on deed men: and takyth theyr bodyes out of erthe & devouryth them/. . . . It is hiskynde to change sexus/ For he is now founde male and now female: & is therfore an unclean beast as Isyder sayeth/ And comyth to hous by nighte and feyneth mannys voyes as he maye/ for men should trowe it is a man.

St. Albert the Great (Albrecht von Bollstadt, Albertus Magnus) was more skeptical and much more critical than Bartholomew. He was born about 1200 and died in 1280. St. Albert was canonized recently. He was undoubtedly the greatest naturalist of the Middle Ages and his activities were so prodigious and his reputation so great that shortly after his death he became a legendary figure. Supposedly he was a magician, and various books of "secrets" were ascribed to his authorship—e.g., *Secreta Mulierum* and others. His authentic work is voluminous and on many subjects, and it shows

that he was remarkably well informed by the standards of his time. Philosophically he was well in the vanguard, and he did his full share in re-orienting the West from Plato to Aristotle. His biological writings, *De Vegetabilium* (7 books) and *De Animalibus* (26 books), are by far the best of medieval times.

His work on plants was based on the best of the classical authorities, but it also contained many new facts and observations. For example he described with great accuracy the leaf anatomy and venation of the plants which he actually saw. His descriptions of seeds show that he recognized that they contained plant embryos. He noticed that insects were connected with the production of plant galls, and he was aware of the effects of climate on plant distribution. He was especially interested in the problems of plant propagation and plant reproduction and he discussed at length the resemblances and differences between the sexuality of plants and animals. He believed in spontaneous generation, as did his classical precursors. Like them, he held that what corresponded in plants to the sexes in animals was fused together and appeared in a single individual. The animals, however, he held to be the more "perfect" because they required two individuals for the sexual act.

He was aware, however, that the date palm was dioecious and reproduced as did the animals. His terms unfortunately are somewhat ambiguous and his "male" and "female" plants, in such genera as the peony, are either different species or different varieties. He also thought that certain plants grew from a single seed but that others had to grow from a number of seeds that had to be planted together so that they could fuse. This seemed to him to be a crude kind of sexuality.

About three-fourths of St. Albert's work on animals is taken either directly or indirectly from Aristotle, but the remainder is, in large part, his own. He described the embryos within the eggs of birds and fishes whose development he could study easily. He was also able to contribute to animal geography because he recorded all of the animals that he saw on his various journeys about Europe. In addition to his own observations, he relied on the observations of others. He gave excellent accounts of whaling and of walrus hunting in the northern seas. But more important than this, he rejected many of the current zoological superstitions. He denied that barnacle geese grew on trees (a notion that lasted until late in the sixteenth century), and he doubted the existence of the griffin. He

stated that the beaver (Castor) did not castrate itself, that the salamander did not live in flames, and that the phoenix did not arise from the ashes of its own funeral pyre. With St. Albert, zoology returned from the level of Aesop's fables to the status it had achieved with Aristotle.

St. Albert was especially interested in the problems of genetics and he sought to discover the mechanism that caused children to resemble their parents. In particular, he recorded the transmission of hereditary defects. Naturally he believed that acquired deficiencies were inherited, and he sought to find the mechanism of this inheritance in the origin of semen. He quoted Hippocrates, Empedocles, and Anaxagoras, and his description of pangenesis anticipated Buffon's in almost every detail. In spite of his mistakes, St. Albert was bringing brains back again into biology.

Thomas of Cantimpré (*ca.* 1204–*ca.* 1280) was almost an exact contemporary of St. Albert. He was a Fleming who wrote a very popular encyclopedia, a work that bore the all-inclusive title *De Natura Rerum.* It seems to have been a very influential textbook and was translated into Flemish and German. Its very popularity indicates that Thomas was not too advanced, but that he was a real man of his time and as such, of course, was very credulous.

The Nature of Things opens with a treatise on the human body. Here the different organs, their supposed functions, and the several parts of the body are described separately, each one in a short section. This first part is followed by a treatise on the human soul and on the strange races of man, hermaphrodites, gymnosophists, dwarfs, etc. Next come books devoted to quadrupeds, birds, marine monsters, fishes, snakes, and worms.

The botanical part consists of books on trees, on aromatic and medicinal plants, and on herbs. These biological books are followed in turn by books on fountains and rivers, precious stones, metals, the sphere and the seven planets, meterology, the universe, and the four elements.

The whole work covers some six hundred short topics. Its value to us lies in the fact that it gives us an excellent picture of the beliefs and the general intellectual standards of the thirteenth century. The truly great men of this century, such as St. Albert, were well in advance of their contemporaries, but Thomas belonged in his times.

Roger Bacon (1214–1292) was ten years younger than Thomas

and several hundred times as famous; so famous in fact, that his real abilities and accomplishments are obscured. For centuries he was neglected by the learned world, but his reputation somehow penetrated to the unlearned, among whom he acquired the reputation of being a powerful magician, a man who could work wonders. Various apocryphal books of secrets are ascribed to him. In modern times, however, his reputation has changed, and now he is regarded as having been a modern experimental scientist, who somehow or other was born some several hundred years ahead of his time. Again, his reputation is not deserved. He almost certainly did not invent gunpowder, although in his *Opus Majus* (Pt. VI. chap. 12, ex. 3, tr. Burke, p. 629) he stated:

> We have an example of this in that toy of children, which is made in many parts of the world, namely, an instrument as large as the human thumb. From the force of the salt called saltpeter so horrible a sound is produced of so small a thing, namely, a small piece of parchment, that we perceive it exceeds the roar of sharp thunder, and the flash exceeds the greatest brilliance of the lightning accompanying the thunder.

Roger wrote of the virtues and usefulness of "experimental" science, but there is no evidence that he himself ever experimented. Indeed, it is clear that he had no real concept of what modern experimentation means. He thought that observations, interpreted in the light of reason, would be a useful adjunct to revelation in the search for truth, but he held that any experiment that contradicted revelation had to be faulty. He was as learned and as well acquainted with classical and Arabic science as any of his contemporaries, and he preached the importance of learning the eastern languages. But the importance of linguistic studies, he held, lay chiefly in the opportunity they gave of converting the easterners to the only true Christianity. He was, however, an enemy of the excesses of scholasticism.

Perhaps some of Roger's reputation today is due to the fact that he fell into disrepute with his superiors (after the death of Pope Clement IV, who had requested that he send him all of his philosophical writings). Roger has also acquired the prestige of having suffered house arrest from 1278 to 1292. Nordenskiöld has even gone so far as to write (p. 80), "His liberal views, however, gained for him bitter enemies, and once he was arrested and had to spend years in prison, being deprived of every possibility of working until he was again released." Roger, however, did not

have liberal views. On the contrary, he was a conservative, who adhered to the philosophical doctrines of Plato, while the rest of his world was shifting to Aristotle. At no time was Roger an enemy or even a critic of the religion of his time. He seems merely to have been a quarrelsome man who offended the wrong people.

Roger's interests in biology were only incidental and peripheral, but, because of his present legendary status, it would be well for us to examine them briefly. He took both his botany and his zoology from Aristotle (including Nicolas of Damascus), and from the Arabs, but he never made any biological observations of his own as did St. Albert. In his account of the Amazons, which follows, he showed that he was not free from the all-pervading credulity (from the *Opus Majus*, Pt. IV. p. 378, tr. by Burke):

> For the Amazons, as Ethicus states, were women leading a great army collected from women without men. The Amazons, calling men to them at certain times of the year, conceived; but they slew the male children when born, reserving the females, whose right breasts they cut off by the art of surgery in their youth, that they might not be hindered by the breasts in shooting arrows. From their youth they nourished at their own breasts minotaurs and centaurs, most savage monsters. Hence these creatures used to precede the Amazons as though the Amazons were their mothers, and the Amazons overwhelmed every army more by means of these monsters than by arms.

Roger Bacon accepted the claims of astrology and believed that the stars influenced all forms of life. He believed that the sun, in particular, operating upon corruption, was able to generate life. In fact, he held that the sun had some of the properties of semen. The changing position of the heavenly bodies, however, were not responsible for all the changes that took place in the organic world, and he denied specifically that they were responsible for the degeneration of the human species. He held that the deterioration of mankind was the result of the accumulated effects of the inheritance of sin, because he knew that sin was debilitating, and that children born from sinful parents were naturally weakened. Lamarck himself could not have been more precise. Bacon explained how it was that men no longer lived to reach the ripe old ages of the antediluvian patriarchs (*Opus Majus*, Pt. VI, chap 12, ex. 2):

> Very rarely does it happen that anyone pays sufficient heed to the rules of health. . . . Therefore fathers are weakened and beget weak sons with a liability to premature death. Then by neglect of the rules of health the sons weaken themselves, and thus the son's son has a doubly weakened constitution, and in his turn weakens himself by a disregard of these rules. Thus a weak-

ened constitution passes from father to sons, until a final shortening of life has been reached as is the case in these days.

Roger Bacon took Aristotle as his authority in his account of the origin of semen. Semen was merely the excess food that had been digested in the body. Thus, it was not composed of gemmules that were separated off from the body itself because if it were, the production of semen would be accompanied by pain. This, he held, was not the case. He was a precursor of Buffon's pangenesis rather than Darwin's.

But semen, he held, need not always be necessary for fertilization. Life could be generated spontaneously and also, we might almost say, semi-spontaneously when, because of the fertilizing power of the air, females reproduced without any male assistance. The following passage is a vague adumbration of panspermy. (*Opus Majus*, Pt. VII, sect. 1):

> The human mind can be influenced to accept the truth of the virgin birth, because certain animals remaining in a state of virginity, conceive and bear young, as, for example, vultures and apes. . . . Moreover, Mares in various regions conceive by virtue of the winds alone, when they desire the male. . . .

Animals, according to Roger, were endowed with an instinct which governed their actions so that they were adapted to their surroundings. Thus they did not have to rely on reason.

Compared with Roger Bacon, Vincent of Beauvais (d. 1264) seems a mere hack, but a very industrious and useful hack. Europe was still in the process of assimilating the ancient learning, and Vincent did his full share in broadcasting the knowledge that was recovered. He started the compilation of the greatest of all Latin encyclopedias, the *Speculum Quadruplex Naturale, Doctrinale, Moreale, Historiale.* It was completed in its original form between 1244 and 1254, but it soon included excerpts added from later works. Part of it, at least, was finished in the next century. Vincent contributed nothing new to the biological knowledge of his age, but he made this knowledge available to others. His method was to take a biological topic and arrange under one heading all that he could find about it in the literature, quoting from Latin, Greek, Arabic, and Hebrew authors. The fact that his authorities contradicted each other was, as far as he was concerned, unimportant. He quoted extensively from them all.

He was not completely uncritical, however, and he denied for example that mutilations were inherited. But he admitted the in-

heritance of gout. We need not repeat here the reasons he gave for his conclusions. They were the usual ones of his age and were based on an almost complete ignorance of the physiology of reproduction. In brief, it can be said that almost every statement we can find anywhere in medieval biology we can find also in Vincent's *Speculum*.

We may bring our account of this remarkable century in Latin Europe to a close by referring briefly to the work of one of the greatest intellects of medieval times, that of St. Thomas Aquinas (1225–1274). The overall philosophy of St. Thomas need not concern us here, nor could we in any brief space do justice even to his biology. His thinking was remarkably critical and logical, but it was based upon the scientific postulates of his age, and his conclusions, obviously, could not be better than his postulates. No selection of short excerpts as examples of his biological orientation could be completely fair to him. To be just to St. Thomas, we should acquaint ourselves with his whole treatment, because his interest in biology, and consequently his emphasis, is quite different from ours. His interest in the mechanisms of heredity, for example, was due in large part to his efforts to explain how original sin was inherited, and a four-thousand-word essay on the origin of semen that he included in his *Sententiarum* was designed primarily to explain this inheritance. He was also concerned with the problem of whether the soul of a child was traduced from the soul of its parents, and he decided that it was not. Mental defects that were alike in father and son, he held, were due merely to the transmission of defective matter—that is, to the defective carrier of the immortal soul. (From the *Summa Theologica* (Bk. II, Quaest. 81, Art. 1):

> . . . . Thus a leper may beget a leper, or a gouty man may be the father of a gouty son, on account of some seminal corruption, although this corruption is not leprousy or gout. . . .
>
> But all these explanations are insufficient, because granted that some bodily defects are transmitted by way of origin from parent to child, and granted that even some defects of the soul are transmitted in consequence on account of the defect in the bodily habit, as in the case of idiots begetting idiots; nevertheless the fact of having a defect by way of origin seems to exclude the notion of guilt, which is essentially something voluntary.

This passage is one of the earliest that recognizes the inheritance of a mental defect.

To journey from thirteenth century Europe to Islam was to move backwards several centuries. The Arabic domination of learning

was now a thing of the past. This does not imply, however, that the Arabic scholars had lost their intellectual standards or that they had ceased their scholarly labors. Their standards just did not improve nor did they keep pace with the ever increasing store of knowledge. Every now and then, however, a scholar or physician would discover something new, causing their factual information to increase somewhat. But it increased very slowly. The Muslims fell behind. The writings of Al-Qazwini (1203–1283) illustrate how far they would have to go to catch up with the Latins.

Al-Qazwini, a Persian who wrote in Arabic, has been called the Muslim Pliny, and he did resemble Pliny in two important respects: (1) he wrote on practically everything, and (2) he was completely uncritical. His two chief works were a geography (*On the Marvels of the Countries*), and a cosmography (*On the Marvels of Created Things*). Two short quotations from the first of these will illustrate the extent of his credulity. His beliefs were shared by other scholars and, in fact, were generally accepted by the learned world. (From the translation of Ferrand):

The Isle of Women in the China Sea. Women are found there and not a single man. They conceive by the wind and bring into the world females like themselves. It is also said that they conceive by eating the fruit of a tree that grows on their isle. They conceive and bring females into the world.

Al-Qazwini also thought that, because of its sexuality, the date palm was the connecting link between the animal and plant kingdoms.

The Date Tree. This blessed tree is found only in the lands where Islamism is professed. The prophet has said, in speaking of the date tree, *Honor the palm tree, which is your paternal aunt:* and he has given it this denomination because it has been formed from the remains of the clay from which Adam was created.

In sharp contrast with the credulity of Al-Qazwini stands the critical writings of a very famous Arabic physician, Ibn-an-Nafis (*ca.* 1208–1288). We need not be concerned here, however, with his many writings on medical topics, but only with one of his anatomical discoveries. It is one, however, that shows that he was one of the greatest physiologists of the Middle Ages. In his commentaries on the work of Ibn-Sina he challenged the prevailing views (of Ibn-Sina and Galen, etc.) that the blood could pass from the right to the left ventricle of the heart, through either visible or

invisible pores in the septum. He stated precisely that it had to pass through an artery to the lungs, where it was mingled with air. From the lungs it passed through a vein to the left ventricle. Obviously he is one of the most important of the precursors of Harvey. He succeeded in describing the pulmonary circulation of the blood some 265 years before Servetus did.

The intellectual ferment of the thirteenth century seems to have missed Byzantium completely. No biological work that is worth mentioning came out of the Greek world. The ferment was working, however, at the other end of the great Eurasian continent. In 1256, Ch'en Ching-i completed a botanical encyclopedia in 58 books. The first part, in 27 books, was devoted to flowers, while the last part, in 31 books, covered fruits, plants in general, herbs, trees, agriculture and sericulture, and food and medicinal plants. The author's interests were broad; he was as much concerned with history and literature as he was with botany. He described each plant twice, first in prose and then in poetry. This work apparently has never been printed, but it exists in numerous manuscripts. It is clearly a forerunner of the great Chinese biological works that were to appear in the next century.

Following the thirteenth century, the fourteenth is somewhat of an anticlimax. It has been both neglected and underestimated. It is true that during this century the Latin West did not make the spectacular advances it had made during the preceding hundred years, but it did consolidate its gains, and it did build a foundation firm enough to support the unprecedented progress of the coming Renaissance.

The Muslim nations, however, were now stagnating. Arabic scientists were no longer in the vanguard, while the Byzantines, on their part, had almost completed their long, slow process of dying. In the middle of the next century, Constantinople would fall to the Turks, and the Greek world would come to an end. The Byzantine libraries were still rich in Greek manuscripts, but few first-class minds were interested in them. The manuscripts would recover their value only when they were carried to Italy and the West. It is worth recording that the scientific and intellectual interests of the Byzantines had started their long, uninterrupted decline some centuries before their nation disappeared as an independent state.

The Chinese, on the other hand, were active in many scientific

fields. In biology, they led the world by a considerable margin. We know that their science was excellent, but we still do not know just how good it was because a considerable portion of it has not yet been studied adequately.

We can begin our account of the biology of the fourteenth century with the *Ruralium Commodorum Libri XII* of Peter of Crescentiis (*ca.* 1233–1321). Peter was a wealthy landowner who had studied logic, medicine, natural science, and law. He became a judge and served as a *podesta* in a number of cities in northern Italy. In 1299, he retired from his judicial duties and returned to his estates, where he completed his famous work on rural affairs in 1305 or 1306. Like Cato, he made his contribution to biology when he was an old man.

Without doubt this was the most important European work on agriculture that appeared between the classical contributions of the Romans and the Renaissance books on husbandry. It exists in numerous manuscripts and it has been printed over sixty times—in Latin, Italian, German, French, and Polish. It was being printed as late as the seventeenth century. Many of the later editions were thoroughly edited and elaborated. Some of them even contained directions for growing the American plants that reached Europe two centuries later. The importance of the work is illustrated by the fact that it practically dominated the field for about four hundred years.

Basically, the *Ruralium Commodorum* was a compilation, but its sources were many. Peter took his material from the Romans, the Arabs, and from the medieval Latins. He was especially indebted to St. Albert the Great for his botany. His work covered all of the practical problems of agriculture, such as the different types and sources of manures, methods of harvesting and storing crops, etc. He emphasized, however, the more biological aspects of farming. He described many kinds of vines, fruit trees, and useful shrubs. The section on gardens contains, perhaps, his most original contribution. It includes directions not only for growing culinary vegetables but also for raising medicinal herbs. His book on domestic animals, fowls, and bees is exceptionally inclusive and contains much veterinary medicine. Peter tells us in detail what was known or believed about all the domestic animals and plants. He also gave an account of the best techniques of hunting and fishing. Peter should not be credited with or blamed for much that appeared in the numerous

later editions of his work. These editions, however, tell us a great deal about what the various editors thought at the time.

At this time, Italy was the cultural leader in the west and the Italians had become relatively active in the sciences. Their interest in biology, however, was still concentrated on its agricultural and medical applications. At the beginning of the century, Peter had published his great work on agriculture, and numerous physicians were writing on the medical sciences. As we would expect, most of the works were mere compilations, but some showed a refreshing originality. The best of these, certainly the most famous, was the *Anatomia Mundini,* published by Mondino d'Luzzi in 1316. Mondino d'Luzzi (*ca.* 1275–1326) has been called the restorer of anatomy. His work was exceptionally influential until the middle of the sixteenth century. He may well be considered the leading anatomist between the times of Galen and of Vesalius. Both his contributions and his limitations are important, because they help to clarify the picture of medieval anatomy. Mondino's scientific standards may well seem incredible to modern scientists.

In 1315, Mondino dissected the bodies of two women, one of whom had menstruated, and also the body of a pregnant sow. The organization of his work shows the handicaps under which the medieval anatomists had to labor. They had inadequate means of preserving their cadavers, so they had to dissect first those organs that would spoil most rapidly. Mondino used preparations dried in the sun to show the positions of tendons and ligaments, and he macerated organs so that he could trace the nerves all the way out to the nerve endings.

Mondino had a technique that could have advanced anatomy by about two hundred years. If he had had the scientific standards that we take for granted today, he could have anticipated Vesalius. But he could not bring himself to challenge any authority whatever. Even when his authorities contradicted each other, he sought to harmonize them. When his own observations could have shown him the errors of Galen and the Arabs, he chose not to see what he himself had laid bare. Thus he continued to describe the stomach as spherical. He described the human liver as five-lobed like the pig's. He observed many uteri under many conditions, but he still stated that they were seven-chambered. He described the large intestine but omitted the appendix. The list of his anatomical errors could be extended indefinitely, and also the list of his highly fanciful

interpretations of physiological functions. In fact, Mondino gives us, perhaps, our best examples of opportunities missed, and of the blighting effects of too much respect for authority.

Matthew Sylvaticus (d. *ca.* 1342) was also an Italian physician and a botanist. He published his large materia medica, *Pandectae,* in 1317. Judged purely as a pharmacopoeia, and in spite of its size, it was but little superior to those then in use. Matthew gave the names of the plants in Latin, Greek, and Arabic, but in the latter language, very inaccurately. His own botanical work, however, was better. His descriptions of plants were detailed and relatively accurate. He traveled widely and noticed and described the native plants wherever he went. He established a true botanic garden in Salerno, the first of its kind, if we omit the rather common herb gardens. Whatever value his work may have had lay in his genuine botanical observations.

The growing importance of the European vernaculars is shown by the work of a German, Konrad of Megenberg (1309–1374), who is known primarily for his famous *Das Puch der Natur,* the first work of natural history in the German language. It was basically a translation from the Latin of Thomas of Cantimpré, but Konrad incorporated many of his own observations. Apparently, he wrote in German to educate the women and the common people, who could speak and read this vernacular only.

Merely listing the contents of *Das Puch der Natur* will show both the ground that it covers and its scientific standards. Book I is divided into 50 short chapters or sections each of which describes the anatomy or the physiology of a human organ. Book II covers astronomy, Book III is zoological and describes 69 quadrupeds, 72 birds, 20 sea monsters, 29 fishes, 37 snakes, and 31 worms. Books IV and V are botanical; the former contains descriptions of 84 trees, the latter of 89 herbs. Book VI covers the precious stones, and Book VII the metals. Book VIII deals with the wonderful properties of streams and with human monsters. *Das Puch der Natur* was exceptionally popular and, beginning a century later, was printed many times.

Konrad avoided a number of the current superstitions concerning animals, but not all. His ideas of hybridization were completely uncritical. He described the leopard as a cross between the lion and the panther, and he told how the viper hybridized with the eel. A brief list of domestic hybrids is given (Bk. III, chap. 34). The

"ibrida" are the offspring of wild swine crossed with tame; a mule is a cross between a horse and an ass; and a goat-sheep is a cross between a billy goat and a ewe, just as a sheep-goat is a cross between a ram and a nanny goat.

Konrad recognized sex in the date palm, but his understanding of it was not at all clear. He stated that the female received from the male only a spiritual force—an air or a vapor—but later he stated that the wind bore the dust of the male to his wives. Sex in other plants was not recognized.

There is little to record of Arabic science during this century. One excellent contribution to agriculture, however, was made by Al-'Abbas al-Rasuli, the Sultan of Yaman, who reigned from 1362–1376. According to Sarton he wrote this remarkable work which still, unfortunately, remains in manuscript. He covered such topics as soils, fertilizers, waters, seasons, and the improvement of land. He described the techniques of raising the grains, the legumes, the cucurbits, and other vegetables. He wrote on spices, perfumes, and fruit trees, and on techniques of vegetative propagation such as by cuttings and grafting. He included all of the known means of preventing plant diseases, and he listed the medicinal properties of plants. The sources of his information were the ancient Greeks, his fellow Arabs, and the experiments he himself performed in Yaman.

During the latter half of the fourteenth century a number of Arabic books on the horse were written. These works on hippology were literary and practical rather than zoological so they need not concern us here. One Arabic theologian and zoologist, Al-Damiri (1344–1405), however, wrote a very voluminous and rambling encyclopedia of zoology. It contained over one thousand articles, but because of duplications it described fewer animals. As a whole the work is a hodgepodge of zoology, of traditions connected with animals, and of the legal status and dietary use of animals. It was enormously popular, and several shorter editions of it were published. To us it shows only that at this time Arabic biology was deteriorating.

How far Byzantine biology had fallen behind the science of the other civilized regions is well illustrated by the zoological work of Manuel Philes, a court poet, who flourished in Constantinople between 1275 and 1345. He wrote poems on many subjects but we are concerned here only with his zoological verses. One poem (381 lines) was on the elephant, another of 2015 lines was translated into

Latin and entitled *De Animalium Proprietate.* It was divided into 103 sections, each of which was devoted to either a quadruped, a bird, or a fish, except for one relatively long section on the Ethiopian dragon and other beasts.

De Animalium Proprietate was derived from the *Physiologus* and from Aelian, and its sophistication was on about their level. It was a strictly moral zoology, little better than Aesops fables. Mixed in among the real animals, Philes included some mythological creatures such as the onocentaur (here described as a kind of tailless ape) and the unicorn. He relied heavily on the folklore of his times both for his facts and for his interpretations. He told how the hyaena changed its sex every year, and how the eel and the viper met at the seashore and there hybridized. Of significance to us is the fact that Manuel Philes' poem on animals constitutes almost the sum total of Byzantine zoology during the entire fourteenth century.

Far removed from Philes both in distance and in intellectual standards was the great Chinese physician, Hu Ssu-hui, who served as the imperial dietician from 1314 to 1330. He wrote a very important book on the principles of correct diet, which he presented to the emperor. He recognized the diseases due to dietary deficiencies and described the two types of beriberi, which we recognize today as the dry and the wet kinds. For the cure of such diseases, he recommended food that was rich in vitamins. The Chinese, of course, had long suffered from periodic food shortages and, being a logical and systematic people, had well-established rules as to what could be eaten when the situation became desperate. It is not surprising that they acquired a great deal of purely empirical knowledge as to the dietary effects of practically everything that could serve as food. Their knowledge of what we may call the secondary value of food plants can be traced back to the second century. But Hu Ssu-hui systematized this knowledge and devised a regimen for the diseases recognized as due to deficient diets. He added the information that he had acquired through his administrative duties to what had long been known.

During the fourteenth century, the Chinese were active in many fields of science. Their biology, medicine, and agriculture were far in advance of their counterparts in the West. Here we will mention the work of but three of the Chinese who contributed to or wrote on agriculture. The first is Wang Chen, who published in 1314.

His work, in twenty-two books, can be broken down as follows: six books on the general rules of agriculture, four books on cereals, and twelve books consisting of pictures of agricultural tools. More remarkable than the contents of this work is the fact that it was printed from movable wooden type. Wang Chen, himself, either invented or improved this kind of type which, incidentally, held the ink and wore better than the contemporary type made from clay or tin.

The second Chinese agriculturist was Lu Ming-shan, who flourished from 1314 to 1330. His work called attention to and corrected a number of errors in the agricultural works that were in general use. In addition, he added new information that he himself had gained. His treatise was essentially a farmer's calendar and he described the work that should be done month by month.

The third agriculturist, a woman, Huang Tao-p'o, who flourished early in the century, may be in part mythological. She is credited with having brought cotton to China. Cotton had been grown in southern China even before the fourteenth century, so perhaps the most that we can credit to Huang Tao-p'o is an improvement in the methods of growing and weaving cotton. It is possible that she taught the Chinese the better cotton-growing techniques that had been developed in India.

Chu Hsiao (d. 1425) was the fifth son of the Emperor of China and is remembered today for the remarkable herbal he wrote, the *Famine Herbal.* From 1382 to 1400, he lived on his estates where he established a botanic garden. Here he experimented with importing and acclimatizing wild plants. Unlike the earlier Chinese herbals, which emphasized medicinal plants, his *Famine Herbal,* as its name indicates, was devoted chiefly to plants that could be eaten in an emergency. Of the 414 species it contained, 276 were described for the first time. The plants were described from actual specimens grown in his garden and were illustrated by drawings made directly from nature.

The *Herbal* was printed in 1406, half a century before printing was developed in Europe, and the illustrations in it were printed from wood cuts which were made with the characteristic Chinese artistry. They are as good as those used in the West a century later. The objective of the *Herbal* was entirely practical; it was to enable more people to survive the periodic famines.

It might be worth mentioning here that the *printing* of herbals

in China goes back to a very early date. The earliest printing of such books occurred supposedly in 973. Other printings have been recorded as of 1101, 1159, etc. The iconographic tradition of the *Famine Herbal* probably goes back to these earlier works.

In the fifteenth century, the European Renaissance began.

7

Daybreak Over Europe

During the fifteenth and sixteenth centuries, Europe awoke intellectually. Here, in the old Latin West, the sciences began their exponential rate of growth—the rate they have maintained ever since. Almost immediately, Europe left the rest of the world behind. Soon the other centers of civilization were so far outdistanced that, for the next four hundred years, the history of science would be the history of European science. Many causes have been assigned for this rebirth of the intellect in Europe, and for the loosening of the medieval bonds that for so long had hindered the acquisition of knowledge. Although we can recognize some of the factors responsible for this almost explosive progress, the whole story still evades us.

But the facts themselves are clear. Major "breakthroughs" occurred in almost every sphere of human activity and interest. Europe seemed to have acquired an almost unbounded supply of energy—enough, at least, to start a new epoch in the history of our species. It was at this time that the unique culture we call "Western civilization" began—the culture that was founded on a combination of technology and scholarship, the culture that was so effective that it altered the ideals, the standards, and the expectations of the human race.

Classical learning was recovered in a few short years; but, even during its recovery, it was transcended. Almost immediately, the men of the Renaissance advanced well beyond their classical preceptors. Evidently they were in a far better position to augment

the sciences of antiquity than were the men of antiquity who had created the sciences in the first place. In the Renaissance, classical learning was soon placed upon a foundation that was much more reliable than the one it had rested on in antiquity.

Although higher learning had been retrogressing in the West for over a thousand years, the humbler arts and the mechanical techniques had not deteriorated in any way, nor had they even remained stationary. They had, in fact, been making spectacular progress. The flying buttresses of the medieval cathedrals showed the unprecedented competence of the medieval architects, and the deeper mines, especially those in Germany, gave proof that major advances were made by the mining engineers. The improvements in mining also gave the West an adequate supply of metals, perhaps for the first time in history. Wind and water mills had been known to the ancients, but by the time of the Renaissance they had developed far beyond their classical prototypes. Then too, the fifteenth and sixteenth century Europeans had larger and stronger horses, and better designed harnesses—harnesses that did not pinch off the horse's windpipes as they pulled heavy loads. This gave transportation a great increase in horse power. Technically, the Europeans were far in advance of the ancient Greeks and Romans.

The ships of the fifteenth and sixteenth centuries were also superior to those that had linked the Roman Empire together. While the art of sailing against the wind had been discovered in classical times, its full potential was not developed until much later. Although the ancient Mediterranean merchant ships were fit for ocean voyages, even as early as the time of Carthage, they were not as well adapted for prolonged explorations as were the vessels of the Portuguese and Spanish adventurers.

The Renaissance Europeans had also acquired valuable techniques from the Arabs and Chinese—techniques that their classical precursors had lacked. During the long medieval night in which the learned men of the West had hibernated, the Arabs and Chinese had been active and productive. A number of their more useful inventions had diffused westward into Europe. As we have stated earlier, "Arabic" numerals had spread from India. The art of papermaking, which had originated in China, reached Europe in the twelfth century, and the possession of paper soon freed the Western scholars from the limitations imposed on them by the scarce and expensive parchment.

The art of printing with movable metal type appeared suddenly in Germany and the Netherlands at about the middle of the fifteenth century. Indeed, this kind of printing may well have been invented in Europe, although it had already been in use in China for about a hundred years. The Chinese had been printing from movable earthenware type from about 1045 on, and from wooden blocks from about the eighth century. We cannot be sure that printing developed independently in Germany and the Netherlands, however, because the *idea* of printing could have diffused along the same route as that of papermaking.

Two more technical innovations added tremendously to the equipment and strength of Renaissance man. Significantly enough, both had appeared earlier in China. The first was the mariner's compass. This device was in use in the twelfth century both in Europe and in China, but the suspended magnetic needle had been known in China a century earlier.

The second invention was gunpowder, definitely a Chinese creation. In the twelfth century it was used in China for rockets and in the thirteenth for artillery. In Europe, it was described in the thirteenth century and used in battle in the fourteenth.

In the fifteenth century, several events that might almost be described as historical accidents influenced profoundly the development of European science. In 1453, Constantinople was captured by the Turks and the Byzantine Empire came to an end. From this point on, Greek science ceased to exist. The victorious Turks invaded Europe, captured Hungary, and were repulsed only at Vienna. Later the Turks retreated and Hungary was freed. This invasion marked the last retreat of Europeans—even their last temporary withdrawal—until the present century. And before the fifteenth century ended, the western peoples had begun their unprecedented expansion—an expansion that continued into the twentieth century.

Before and after the fall of Byzantium, a number of Greek scholars took refuge in the Latin West. For the first time since the collapse of Rome, the Latin world had full access to a number of important Greek manuscripts and to scholars who could translate them. The classical works that the West had known only in Arabic translations they now had in the original Greek. Theodore Gaza (d. 1478), a scholar who had left his native Greece in 1430, translated Aristotle and Theophrastos into Latin. The European scholars now had the "authentic" texts, with the happy result that there was an

intense interest in all things classical, and a tremendous development in textual criticism.

This was very good indeed. Many of the works that hitherto were unavailable were now spread broadcast by the new art of printing. Classical science was recovered, but the "authentic" translations did not advance science quite as much as the scholars thought they did. The value of the authentic works of the ancients was greatly overestimated, which was only natural. We must remember that even as late as the fifteenth century scientific questions were settled by authority and, with such standards, it seemed to be of the utmost importance for the scholars to discover just what versions of the classics were authentic. There are many good reasons, of course, why we should know just what the classical scientists believed and wrote, but we no longer have to recover their notions in order to advance our science. Now we know that Aristotle made just as many errors in the original Greek as the Latins found in the Arabic translations. The contributions to scientific progress made by translating directly from the Greek sources were themselves only indirect. They stimulated authentic scholarship and increased greatly the general interest in science and its problems, but they added little that was new to scientific knowledge.

The last decades of the fifteenth century saw the beginnings of the great European explorations. In 1488, the Portuguese under Bartholomeu Dias rounded the southern cape of Africa and opened up a new road to the East. Ten years later Vasco da Gama reached India. In 1492, the last of the Moors were driven out of Spain and Columbus discovered America. From now on, strange new animals and plants would reach Europe in ever increasing numbers. The medicinal and culinary plants of the entire world would soon become available to Europeans.

Original contributions made to biology in the fifteenth century were not immediately evident. They were obscured in part by the rapid replication of the older works through the activities of the European printers. Any new scientific work had to meet the very formidable competition of the newer and more authentic editions of the classics. It was not until the next century that the full value of printing for spreading new scientific discoveries was realized. Some encyclopedic works such as the *Cornucopia* (1480) of Nicolaus Perottus were printed late in the century. So also were some of the encyclopedias we have cited earlier, such as those written by

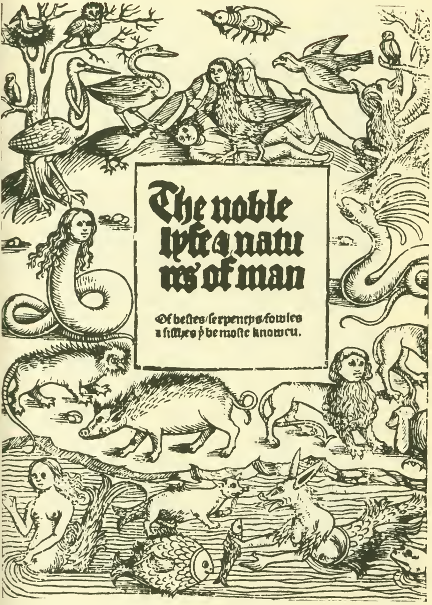


Figure 3. Frontispiece of *The Noble Lyfe and Natures of Man* (Antwerp, 1521), an early English version of the *Hortus Sanitatis*.

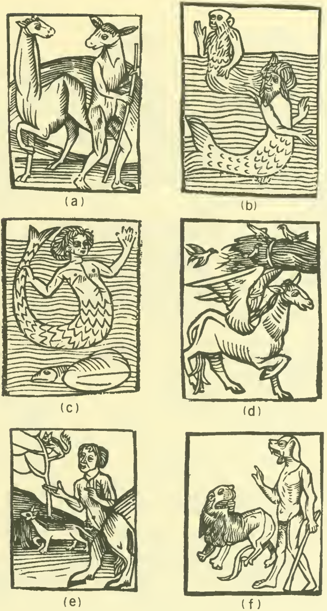


Figure 4. Mythological animals from *The Noble Lyfe and Natures of Man;* (a) Oraffius and Onocenthaurus (man with asses head); (b) Dreyden; (c) Platanista; (d) Stuciocamelo (Struthiocamelus or ostrich); (e) Pylosus; (f) Cephas (with stick) and Centiocata.

Bartholomew the Englishman (1472) and Konrad von Megenberg (1475). At this time also, the first very crude herbals were printed —e.g., the *Herbarius zu Teutsch* (1485), the *Gart der Gesundheit* (1485), and the *Ortus Sanitatis* (1491). (Figures 3–5, 7.)

However, it is ironical that, during the time when so much of the ancient work was being recovered, a most important and original contribution was overlooked entirely. It was a contribution, moreover, that was made by one of the greatest men of all time, the universal genius, Leonardo da Vinci (1452–1519). Leonardo's major and unprecedented contributions to anatomy languished in manuscript almost unknown until late in the nineteenth century. If Leonardo's work had been known and appreciated when it was first done, the life of the man who founded modern anatomy, Andreas Vesalius, might have been much more peaceful.

Leonardo was the illegitimate son of a Florentine notary and a peasant woman. Immediately after his birth his parents were married, but not to each other. Leonardo was raised by his father and educated both as an artist and as an engineer. He soon showed, however, that he was interested in literally everything—in all of the arts and in all of the sciences.

Again, it is worth emphasizing the versatility of those exceptional individuals who added so much to our biological knowledge. Leonardo was a painter (perhaps the greatest), a sculptor, and an architect. He was also a civil and a military engineer. His scientific interests were distributed over mathematics, meteorology, astronomy, physics, geology, physical geography, and biology. In the latter science he made contributions to natural history, paleontology, physiology, and botany. But his most valuable scientific work was in human and comparative anatomy.

He probably dissected animals and almost certainly dissected human cadavers. Otherwise his very accurate drawings of the internal organs would have been miraculous. His illustrations were far superior to any that had ever been done before. He was especially interested in the organs and the mechanisms of reproduction, and, for very practical reasons, he was interested in the exterior details of the human body. As a painter and a sculptor he had to know the exact proportions of the body, its musculature, and its surface markings. His studies of human postures, and of the skeleton and its muscular attachments, however, show the interest of the engineer (Figure 6).

Figure 5. Anatomical figure from *The Noble Lyfe and Natures of Man.*

Leonardo's botanical observations were concentrated on the anatomy of plants, and he described accurately the arrangements of leaves, the location of buds, and the angles made by the various types of branches. His interests here seemed to be in the problems of structural engineering that the plants had solved. He studied intensively the flight of birds, and, again, his interests were those of an engineer. He designed a flying machine, based upon the wing action of birds. But here he was foiled; he lacked an adequate source of power.

Great as was Leonardo, he remained a man of the fifteenth century. A scientist, a practical engineer, an artist without peer, he retained more than a trace of the mystic in his make-up. His notebooks are full of prophecies, most of which have little meaning.

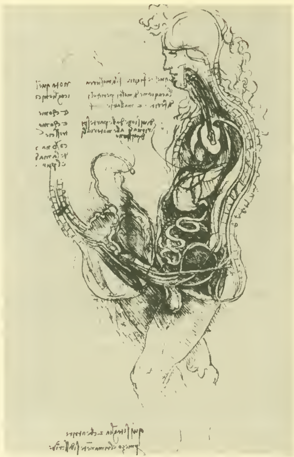


Figure 6. Longitudinal section of male and female in coition (Leonardo da Vinci). Ducts leading from the testes are for the material component of the semen, and from the spinal nerve for its spiritual component. (Reprinted from *Leonardo da Vinci on the Human Body* by C. J. O'Mallen & J. Saunders, by permission of Abelard-Schuman, Ltd. All Rights Reserved. Copyright year 1952.

Some, however, have given him the reputation of a seer. In this one field, perhaps, he is overestimated. He made so many prophecies, and on so many subjects, that some of them could hardly have failed to have come to pass.

In addition to his sound zoological researches, he recorded in his notes approximately a hundred of the then current and routine

zoological fables—fables on the level of the *Physiologus*. How much of the fabulous he really believed we do not know, but he obviously thought the fables important enough to go into his notebooks. The following examples are typical (chap. 43, tr. by E. MacCurdy):

Ingratitude. The pigeons serve as a symbol of ingratitude; for when they are of an age no longer having need of being fed, they commence to fight with their father, and the combat does not end until the young one has driven his father out and taken his wife and made her his own. [Someone must have told this to Freud.]

Deceit. The fox when he sees a flock of magpies or jackdaws or birds of this kind, instantly throws himself on the ground with mouth open in such a way as to seem dead: the birds think to peck at his tongue and he bites off their heads.

Temperance. The camel is the most lustful animal there is and will follow a female a thousand miles, but if it lived continually with its mother or sister it would never touch them, so well does it know how to control itself.

Partridge. This changes from female to male and forgets its former sex. Out of envy it steals the eggs of the others and hatches them, but the young ones follow their true mother.

Leonardo's notes on anatomy were on a much higher level, but even here his technical equipment as an artist sometimes led him astray. He cited evidence to prove that both sexes contributed semen to the formation of the offspring (p. 173):

The black races in Ethiopia are not the product of the sun; for if black gets black with child in Scythia, the offspring is black; but if a black gets a white woman with child the offspring is gray. And this shows that the seed of the mother has power in the embryo equally with that of the father.

As every painter knows, a mixture of white and black pigments produces a gray.

Leonardo accepted the pangenetic origin of semen which, as we have mentioned, had been accepted routinely for the past two thousand years. He was much too good an anatomist, however, to follow the very crude notions as to *how* the semen reached the testes (p. 190):

Hippocrates says that the origin of our semen is derived from the brain, and from the lungs and testicles of our forefathers where the final decoction is made; and all the other members transmit their substance to this semen by sudation, because there are no apparent channels by which they could arrive at this semen.

Leonardo personally examined the anatomy of both man and beast, and his comments and unsurpassed pictures give ample proof of his keen observation. The logical way in which he turned his

observations to account showed that he had the potentialities of a great scientist, but he elaborated only the material which attracted him. In this respect his artistic genius suffered from "les défauts de ses qualités"; he was not prepared to deal systematically with the whole of human anatomy. For details he surpassed Vesalius and Calcar both as draftsman and as observer. Vesalius and Calcar however, served science, as Leonardo served art. Vesalius was the sober scientific student of the human body; Leonardo was an anatomist by the grace of God. Leonardo might have been the founder of biological anatomy, but he did not possess the tranquility and concentration of interests essential to this end. We even have some evidence that Leonardo looked upon himself primarily as a military engineer. He also invented a shorthand, and he wrote backwards in his notebooks. He was truly a universal genius.

In the thirteenth century the orientation of Western philosophy had shifted from Plato to Aristotle, but the Aristotelians soon became just as set in their ways as the Platonists had been. In the sixteenth century, there grew up a sharp contrast between the dogmatic Aristotelians, who withdrew more and more from the sphere of experimental investigation and who accepted, or at least sought, the authority of Aristotle in everything, and the amateurs who were just as interested in nature, but who, because of their insufficient philosophical schooling, showed the same lack of discernment as did the Aristotelians. On the one hand there was a rigidity owing to a blind acceptance of the pronouncements of Aristotle and Galen, and on the other, a keen burning thirst for knowledge and for a renaissance—hampered, however, by the lack of the critical power needed to evaluate accurately the observations that hitherto had been accepted as facts.

This contrast remained latent at first, since each faction went its own way relatively indifferent to the other. Eventually, however, their differences came to a head through the activities of a most remarkable man, a unique figure in the history of science: Paracelsus (1493–1541), called by his publishers: "Philippus Aureolus Theophrastus Paracelsus Bombastus ab Hohenheim, a doctor in both sciences of healing, master of mysteries, prince of chemists."

Paracelsus had a forceful and brusque personality. He was a fascinating orator, romantic and inclined to mysticism, serious and full of respect for the Bible, independent almost to the point of libertinism, merciless in his attacks on the pseudoscience of later

scholasticism and on the querulous worshippers of Aristotle. He was susceptible to the charms both of nature and of research into nature as a lay science, but he remained full of awe towards the unfathomable depth of nature's mysteries, convinced that it is only given to a few to probe these secrets.

But above all he was convinced that he himself was one of the elect upon whom this rare privilege has been bestowed. All this made him an exceedingly ambiguous figure in biology. He was idolized as a world-conqueror, reviled as a rebel, venerated as a reformer of science, detested as a charlatan, held in awe as a man of steel, despised as an uncouth and unabashed libertine, and both beloved as an orator and hated as a dangerous demagogue. Officially attached as a professor to the University of Basle, he was at pains to instill not only into his students but also into the general public a dislike of the refined humanistic tendencies which under the influence of Erasmus, prevailed at his University.

Paracelsus' father claimed to be an illegitimate son of the noble German family, Bombastus von Hohenheim, and Paracelsus' name, before he tampered with it, was Theophrastus Hohenheim. Not much is known about his mother. Young Theophrastus was educated by his father, but later, as one of the mendicant scholars, he traveled all over Europe—even to Moscow and Constantinople. He studied alchemy under Trithemius; he believed in astrology and despised anatomy, holding that the human body was a microcosm, corresponding part for part with the macrocosm. Thus the liver corresponds to Jupiter, the gall bladder to Mars, the heart to the sun, the brain to the moon (hence lunacy?), the spleen to Saturn, the lungs to Mercury, and the kidneys to Venus. When the planets and organs get into unfavorable positions, illness ensues. But if the illness came from God, the only cure is piety and prayers.

In his controversies, Paracelsus was exceptionally quarrelsome and very crude. He said that his backsides knew more than his scholastic opponents, and that his shoestrings knew more than Galen. Most of his similes are too scatalogical to be printed, even today. Thus, by using the language he had picked up as a student, he became expert at making enemies, a talent which he exercised all his life. Naturally he was continually on the move, never holding any position for any length of time. He was rarely in any serious trouble, however, because he soon learned to leave whatever town he was living in, about two jumps ahead of the police.

Simultaneously an idealist and a realist, Paracelsus gives us in all of his utterances the impression of having a dual nature. Although he was convinced of the value of observation as a basis of knowledge, and stated as much, yet in the same breath he rejected it as inferior. For he claimed that observation—even when based upon anatomic dissection—does not reveal life, because he held that life was nothing but a certain vital fluid, which protected the mortal body from the onslaught of murderous worms and rot and which consisted of a liquid compound of salts. The body was an artificial product and not life.

Paracelsus' writings are numerous and of many types, bearing such extravagant titles as "On Nymphs, Giants, Idiots, Wolfmen, and Sheepmen," "On the House of the Lord," "On Surgery," "On Reproduction." Strange mixtures of mysticism and soberness, completely unscientific, yet compelling through their captivating arguments coupled with an inconceivable lack of clarity and barbaric terminology—yet fascinating none the less!

Taken as a whole, Paracelsus' work was essentially negative and devoid of any positive content. It was critical and destructive—we might say steeped in an insurgent nihilism—although there were passages in it that strike us as extraordinarily modern. His importance does not lie in what he has given us but in the fact that his work gave a new turn to natural science, breaking up its rigidity and opening the way to a new and restored life.

In one respect however, Paracelsus did help to improve the practice of medicine. He insisted on the value of mineral drugs. When we remember that the first *authentic* outbreak of syphilis occurred in 1493, the year in which Paracelsus was born, and that syphilis—the "great" pox, as contrasted with the "small" pox—had spread with devastating effect from Naples over the entire continent of Europe, we can understand the value of mineral drugs. Mercury was used in the treatment of syphilis until the nineteenth century, when it was superseded by the famous arsenic compound. (Whether or not syphilis was brought to Europe from Haiti by the sailors of Columbus is here beside the point.)

Paracelsus was not so fortunate in his treatment of medicinal plants. He accepted completely the doctrine of signatures, a doctrine that would mislead physicians for the next two centuries. In brief, this doctrine is that the Creator of the Universe, at the same time that He created diseases, created the cures for the diseases, and

also gave the necessary clues as to where the cures were to be found. These clues could be recognized by the wise men and the scholars who realized just what God was up to. Thus, for example, any herb that resembled an organ was the proper medicine for any illness of the organ. Hepaticas, or liverworts, were for diseases of the liver, asplenium for the spleen, Hypericum—which had holes in its leaves—was good for wounds from stabbing, and the peony—because its pistil was shaped like the cerebrum—was good for paralysis of the brain. The moonwort was good for lunacy, because its leaves were shaped like the crescent moon, etc.

Paracelsus gives us a strenuous introduction to the sixteenth century, which was to see the awakening of biological interest in many different fields. Philosophy, as the starting point for the investigation of nature, receded into the background, while empirical research—observation and experimentation—advanced, although at first with seeming diffidence. And once again it was medicine which stimulated the interest in biological phenomena. The study of medicine crystallized around two central areas, the first being the study of medicinal herbs and, growing out of this, the investigation of plants not used in medicine.

Human anatomy was the second area for medical research, along with—as an accompaniment to it—the dissection of animals. Animal dissection, however, was resorted to only when it was needed for practice in the art of dissection, or to give an added insight into human anatomy. But in spite of the medical orientation of sixteenth century natural science, biology owes a great deal to the many investigators who labored during that period.

The field of medicinal plants was cultivated by a guild of herbalists. There existed at the time, however, a few rare offshoots of a post-Aristotelian school of philosophy, which influenced the development of botany and pharmacology. There were numerous translations of Theophrastos and Dioscorides. There were also printed editions of Thomas of Cantimpré by way of Konrad von Megenberg (*see* Chapter 6) whose *Puch der Natur*, as we have stated, was printed in 1475. At the apex of these Aristotelians stood Andrea Cesalpino (1519–1603), an Italian physician, pharmacologist, and botanist, whose *De Plantis Libri XVI* (*"Sixteen Books on Plants"*) was published in 1583. This work marked the closing phase of this orientation.

Cesalpino, however, had much to recommend him. He made a

real effort to classify plants, although, as his system was based on a single organ, it was artificial. As an anatomist his work was respectable, and he actually described the *circulation* of the blood from the right side of the heart, through the lungs, to the left side. In this, though, he was following Servetus and his teacher Columbus. Cesalpino, incidentally, was the personal physician of the Pope.

Meanwhile a non-physiological trend had become established in the herbals, a trend which, in a short time, acquired a reputation that superseded the Aristotelian or physiological study of plants. This development was strengthened by an important improvement in the art of printing. The earlier printed books, whose sources were the ancient manuscripts, were illustrated with primitive wood cuts, although they do contain here and there an illustration which had obviously been made by a professional artist—as, for instance, the Iris illustration in the popular *Herbarius zu Teutsch* (1485). (Fig-

Figure 7. "Acorus" (Iris) from *Herbarius zu Teutsch* (1485).

ure 7). But the infancy of wood engraving was passing: the technique of carving the blocks was improving rapidly, and in 1530, when Otto von Brunfels (1464–1534) appeared on the scene with his *Herbarum Vivae Eicones,* he was able to use if not perfect at least wholly adequate plant illustrations—thanks to his collaborator, the artist-engraver Hans Weiditz. These drawings of Weiditz were not copied from old manuscripts. They were fresh and vigorous, as, for example, the beautiful picture of a Christmas rose (Figure 8). Usually each illustration was accompanied by a long verbal description of relatively little scientific value.

Figure 8. Christwurz (*Heleborus niger*). (From Brunfels, *Herbarium Vivae Eicones*, 1530.)

Soon Brunfels' work was followed by that of two other herbalists, the first being Hieronymus Bock (Latinized to Tragus, 1489–1554), a Protestant preacher and physician, who began his publications in 1539 with a volume of plant descriptions without illustrations, *New Kreutterbuch von Unterscheydt, Würckung und Namen der Kreutter* ("New book on the Distinguishing, Effect and Names of Herbs"). He followed this later by two illustrated editions (1546 and 1552) based largely on his personal observations. His notes on the habitats and distribution of plants were excellent, far better than those found in the older works. A few years later (1542) appeared the beautifully illustrated work of Leonhard Fuchs (1501–1566), *De Historia Stirpium* ("On the History of Herbs"), a book in which Fuchs paid homage to his collaborators—the draftsmen Heinrich Füllmauer and Albert Meyer and the engraver Veit Rudolf Speckler —by inserting their portraits. Fuchs' large folio editions of 1542 and 1543 reached a very high technical and artistic standard.

These three men, Brunfels, Bock, and Fuchs, who are referred to as the "German fathers of botany," form an important triumvirate of pioneers. One of them perhaps was better at illustration, while another might excel in accurate description and personal observation. All three are classical figures in botany because they not only turned their attention to medicinal herbs, but also devoted large sections of their work to describing common wild plants. This gave their books the value of being more or less the precursors of the later botanical studies of local floras.

Somewhat later, but of almost equal importance and of equal historical significance, appeared a second triumvirate of botanists, this time in the Netherlands. The leader was Rembert Dodoens, or Dodonaeus (1517–1585). He was joined by Charles de l'Ecluse (Clusius, 1526–1609) and by Matthias de l'Obel (Lobelius, 1538–1616). They set about the compilation of the flora of the Netherlands, adorned with innumerable descriptions of the plants they had collected during their journeys and visits to Austria, Hungary, France, Spain, and Portugal, or which had been brought to them by friends from other parts of the world. Dodoens (Figure 9) may

Figure 9. Rembertus Dodonaeus (1517–1585).

rightly be considered the founder of botany in the Netherlands. His *Cruydeboeck* of 1554 (Figure 10) is one of the most famous of the botanical classics.

Among authors of herbals in other countries of Europe, we may

Figure 10. Title page of Dodonaeus' *Cruydeboeck* (first edition, 1554).

mention the Swiss Konrad Gesner (1516–1565) and Caspar Bauhin (1550–1624); the Italian Pierandrea Matthioli (1501–1577), and the Frenchmen Jean Ruel (Ruellius, 1474–1537) and Jacques Dalechamps (1513–1588). Around these central figures we can group many others of lesser importance, such as Valerius Cordus,

Christian Egenolph, Adam Lonicerus, Peter Uffenbach, Balthasar Ehrhardt, Pierre Pena, Jacob Dietrich (Tabernaemontanus), William Turner, and Henry Lyte. Thus in the first half of the sixteenth century there was a remarkable manifestation of interest in plants —a sudden, almost spontaneous, growth of botanical study that took place over practically all of Europe. This interest in plants was to continue throughout the whole sixteenth century.

That such an increase in interest should have sprung up so suddenly was undoubtedly due to a change in scientific conceptions. That it was able to spread so rapidly over the whole of Europe was due to a cause entirely outside the domain of natural science—that is, to the discovery of printing and to the swift technical advances made in the field, especially in the printing of illustrations. Moreover, along with the technical advances, a marked change occurred in the character of the illustrations, as the more or less successful stylization of the older manuscripts, and in the earlier printed editions based on them, began to be replaced by a realistic representation of the plant in an attempt at faithful reproduction.

The *Herbarium Pseudo-Apuleii* from which Figure 11 is taken contained, in the manuscript, only illustrations which had been drawn so as to be as decorative and symmetrical as possible. In the reproductions made—oddly enough from metal clichés—this character was maintained, thus making it very difficult for us to recognize the plant itself. In later works (Figures 7 and 8) this attempt at artistic symmetry was abandoned and a naturalistic method adopted. Scientific interest had grown; the artistic element remained, but was made subservient to the scientific. The works of Brunfels and Fuchs, in particular, caused a change in this direction, but in this they were no doubt following the precedent that was established in plant illustration by artists of genius like Dürer and Leonardo da Vinci. Also, copper-engraving was slowing coming to the fore—a technique which could give a much clearer picture because of its more delicate finish.

In the course of the subsequent development of botanical illustration, however, the artistic element once more gained ground: the wood-engravers strove to fill up the whole rectangle of the block with the picture of the plant, thus adding undesirable twists and twirls, or surrounding the plant with some decorative frame. The copper-engravers liked to see the plant depicted as part of a landscape, which often brought about curious discrepancies between the

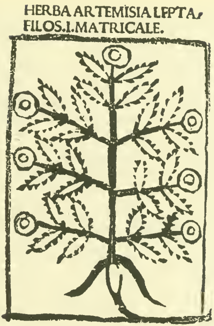


Figure 11. Artemesia (wormwood). (*Herbarium Pseudo-Apuleii,* fourth or fifth century A.D.)

botanical character of the plant and its environment. However, taken all in all, botany owed much of its development during this period to the technique of printing. And it owed no less to the enterprising publishers of expensive works: such publishers as Hans Schotten and Wendel Rihel in Strassburg, Michael Isingrin in Basle, Christoffel Plantijn in Antwerp and his son-in-law van Ravelingen in Leiden, and many others.

From the scientific standpoint also there was a noticeable improvement. Originally the plants were drawn only in their entirety. Later, beginning with Gesner, parts of plants, flowers, and seeds were drawn in detail in addition to the larger drawings of the whole plant.

The scientific treatment of plants in these herbals showed another important development, both in the descriptions and in the classifications. The method used by St. Albert the Great in describing the characteristics of flowers had already shown a striking advance over that of his predecessors. He had distinguished between plants that

had a calyx and corolla from those that had only a perianth. He demonstrated that the successive floral circles, of which a flower consists, alternate. Brunfels had wonderful pictures but incomplete descriptions, while with Bock the illustrative side was weaker but the descriptions were better. Fuchs included in his work a list, incomplete and inaccurate, of the terms and their definitions that he used in his descriptions of plants, as Dodoens did subsequently in one of his later works (*Pemptades,* 1583).

Some writers of herbals liked to express themselves in a poetic and florid style. Others gave a sober account of the phenomena they observed. The climax in descriptive method was surely reached by Caspar Bauhin (1620), as is evident from his description of a type of beet root:

> From a short, pointed non-fibrous root, sprout several stalks eighteen inches in length: they lie along the soil, are cylindrical in form and grooved and near the roots they turn white, dividing into delicate branches with a thin felty covering. Like *Beta nigra,* this plant has only a few leaves but they are small and have very long petioles. The flowers are small and a greenish-yellow; the fruits grow in large numbers, close together and near the root, whence they spread along the stalk of nearly every leaf. They are rough and bladder-shaped, ending in three points which curl backwards. Inside them lies a seed, the shape of an Adonis-seed; it is slightly rounded and ends in a point and is covered with a double layer of a reddish cuticle, the inner one filled with a white floury substance.

Also, during the sixteenth century, the system of classification was radically changed for the better. Generally, the primary division in the ancient herbals had been into trees, shrubs, and herbs. Later, plants in these herbals were either arranged alphabetically or grouped together for some apparently arbitrary reason often incomprehensible to us. Tree fungi were grouped with mistletoe, which also grows on trees—but Bock declared that fungi were neither herbs, roots, flowers, nor seeds. They were simply surplus humours from the earth that collected in rotting trees and in other rotting things. That was why they appeared mostly after rain and thunderstorms.

With the increased number of plants being described—in Fuchs (1542), over 500, in Bauhin (1623), over 6,000—the need for greater order was felt. At the same time, the external similarity between certain groups of plants became more apparent. All the writers of the herbals had a vague sense of the existence of some relationship between plants. Bock wrote that he had arranged together, yet in separate groups, all plants which seemed related or showed some

connection, or which resembled each other, or which might bear comparison. He abandoned the old method of arranging according to the alphabet because this classification gave rise to many ambiguities and errors. Here is the first indication of a *system of plant classification.*

Gesner was the first to define the difference between *genus* and *species* and between *order* and *class.* He held that there was one thing of which we may be quite sure—there were hardly any plants whose genus could not be split into two or more species. For example, the ancients had described one kind of gentian—he knew ten kinds.

Here again the climax was reached with Bauhin who, unlike his predecessors, used a method of classification which was not primarily designed to serve utilitarian ends. In his work, each plant had a name assigned to it according to the *binomial system,* a generic name and a specific name, the latter at times consisting of two or more words but nevertheless constituting a whole. Thus, Bauhin gave good specific descriptions, more or less like our modern diagnoses. He showed that he felt the affinity between these plants by means of common generic names, but he gave no descriptions or diagnoses of his genera, although he did often describe large numbers of plants in sequence—plants which nowadays we consider as belonging to the same family. He did not commit himself, however, as to their possible kinship; only the sequence in which they were mentioned points in this direction.

Most of these herbals impress us by their sober and objective approach as compared to the mystical statements that have reached us from Greek and Roman antiquity; yet in some of the books a great deal of imagination went towards making the drawings, for which a standardized symbolism was used. In the *Ortus Sanitatis,* a popular, anonymous herbal dated 1491, we are told how the youthful Narcissus turned into a flower (Figure 12). Gerard, who a hundred years later (1597) published an English version of Dodoens' *Cruydeboeck,* added the story of the "Goose-tree" or the "Tree of the Barnacles"; he "does not believe in fairy-tales"; he tells us that he recounts only what he saw with his own eyes, namely, "that on rotting trees on the Lancashire coast, grow white shells, formed out of sea-foam; that later, out of those shells issues a form resembling a bird; first the wings become visible, next the legs and when this young bird grows up, it leaves the shell and falls into the sea where

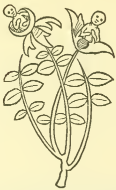


Figure 12. "Narcissus." (From *Ortus Sanitatis,* 1491.)

it grows into a real bird, larger than a duck and smaller than a goose." (Figure 13.)

Figure 13. "The breede of Barnakles" or the "Goose-tree" from Gerard's *Herbal* (1597).

These fantastic stories are fairly harmless in themselves; they appear sporadically in a few of the books. A greater danger lay in the return to mysticism, so that even as late as the seventeenth century whole books were devoted to the astrological significance of

plants and to the so-called "doctrine of plant-signature." Paracelsus had set a precedent in this, Carrichter followed in 1577 with his *Kreutterbuch*, Thurneisser in 1578 with his *Historia*, and Porta in 1588 with his *Phytognomonica*. In these herbals, conclusions as to the healing powers of plants were reached on very slender evidence and without any reasonable grounds. Plants whose fruits are regularly segmented were supposed to heal the bite of the scorpion because it, too, is segmented (Figure 14). The "moonwort," or *Botry-*

Figure 14. Herbs of the scorpion. (From Porta, *Phytognomonica*, 1591).

chium, has leaves that bear an astrological association, and such an association is postulated between the moonwort and the moon. But such extravagance cannot detract from the value, which this impressive series of herbals has for botany as a whole.

This new interest in botany naturally extended to plants from distant lands. On the plants of Africa there was the work of the Moor, Alhasan Ibn Mohammed Alwazzan Alfasi, who later, as a convert to Christianity, was called Johan Leo (1550). On the flora of Asia and the Middle East we have the works of Pierre Belon (1517–1564) and Leonhart Rauwolf (1583).

Belon deserves, perhaps, more than a cursory treatment, not so much because of his ability and his accomplishments—although these were notable—but because he illustrates so well the conditions in which sixteenth century biology advanced. He was born near Le

Mans in France but traveled widely when he was a student. On returning to France, he found several patrons who made it possible for him to journey to Greece, Turkey, Syria, and Egypt.

This simple fact—that a French naturalist traveled to the Near East to study the flora, fauna, and inhabitants of the region, its archeology, and its oddities—emphasizes the great changes that had occurred in the centers of civilization. The Near East, where civilization itself had started, was now so far behind Europe that a naturalist was sent to study the natives of the region, their customs, and their surroundings. And Belon did discover an oddity—an armadillo shell that he had bought in a market in Syria. The armadillo is a native of America!

The fact that the relic of an American animal could be bought in Syria calls attention to the anomalous way in which the valuable American plants and animals spread throughout the old world. The chief path of their diffusion was through Islam. At the time, Islam extended many thousands of miles—from Morocco to the Philippines. Mobility within the Mohammedan world was high because of the many pilgrimages to Mecca.

As a result, once a plant or an animal reached Islam it spread. Indian corn was carried from Morocco to Egypt, where it was grown early in the sixteenth century, and from Egypt it spread quickly to Mesopotamia and to Turkey. The Turks carried it to Hungary, where it was known as "Turkey wheat." This Turkey wheat got into our herbals. The American bird, *Meleagris gallo-pavo,* is actually called the "turkey," and it spread as did the plants. Indian corn also spread to Persia, and from Persia over the old silk route to China. Arab slave dealers carried it across the Sahara into Central Africa. There is little wonder then that the true origin of American plants was not known for a couple of centuries.

As for Belon, his botanical work was minor; comprising only two books: one on trees, *De Arboribus Coniferis, Resiniferis, aliis . . .* (1553)—which was very well illustrated—and a small one in which he protested the neglect of cultivation, *Les Remonstrances sur le Default du Labor et Culture des Plantes . . .* (1558). His books on his travels, however, contain scattered biological entries. His chief interests were zoological. He wrote two books on fish, *L'Histoire Naturelle des Estranges Poissons Marines* (1551) and *La Nature et Diversités des Poissons* (1555). The value of these books is chiefly indirect. Oddly enough he included among the animals he

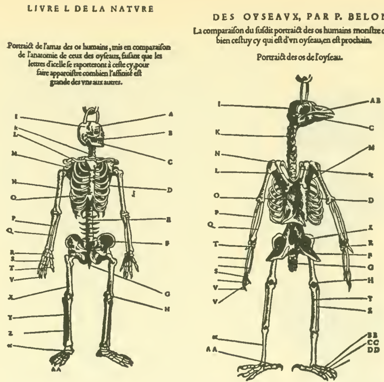
Figure 15. Figures from Porta's *De Humani Physiognomonica* (1586). In this field, the development has been from physiognomy to phrenology to aptitude testing.

classified as fish not only seals and whales, aquatic worms, crustaceans, and mollusks, but also the beaver, the otter, and the hippopotamus. Perhaps this is the best illustration we have of the then current state of animal systematics.

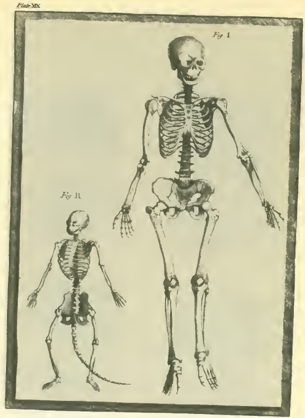
In his *Histoire des Oyseaux* (1555), however, his taxonomy was better. He made a real contribution to the comparative anatomy of birds. He even published a picture of a bird's skeleton and a man's skeleton side by side and pointed out the homologies between their bones (Figure 17). This was the first such picture ever to be printed. Belon had the makings of an excellent comparative anatomist, but he was killed in his forty-seventh year by a highwayman in the Bois de Boulogne.

Figure 16. More figures from Porta's *De Humani Physiognomonica.*

Other European naturalists and travellers also brought a new knowledge of exotic animals and plants back to their home continent. Garcia d'Orta (1563) wrote a book on the simples, drugs, and medical practices of the East Indies, while we get our first information on American flora and fauna from the work of Oviedo y Valdez (1526, 1535) and from the *Badianus manuscript,* an Aztec book of herbal drawings (1552). Nicolas Monardes wrote his famous *Historia Medicinal de Neustras Indias Occidentales* in 1569, which was translated into English by John Frampton (1577) as *Joyfull Newes out of the Newe Found Worlde.* Other Spanish explorers continued to describe newly discovered American animals and plants. In other directions, too, the new advances in botany had far-reaching developments. Professional chairs were created, which, in spite of their



(a)



(b)

Figure 17. Comparative skeletons of vertebrates. (*a*) Skeleton of bird and man from Belon's *Histoire des Oyseaux*, etc. (1555); (b) plate from Bradley's *A Philosophical Account of the Works of Nature* (1721) showing the differences between the skeletons of monkeys and men.

official medical character, were held by professional botanists such as Dodonaeus (1517–1585) and Clusius (1593–1601), who were attached to the newly founded Leiden University.

Botanic gardens, which earlier had served more or less as a private pastime for monks in their monasteries, had been laid out for the use of physicians as early as the fourteenth century. Such was the garden of Gaulterus in Venice (1333). Later, and for specifically botanic purposes, botanical gardens were established by the universities. The first was at Padua (1545), followed by gardens at Pisa (1547), Bologna (1568), Leipzig (1580), Leiden (1587), Heidelberg, and Montpellier (both in 1593). The botanic gardens at Leiden we owe to the initiative of Geraerdt de Bonst (Bontius) and its prosperous condition at the end of the sixteenth century chiefly to Clusius. An old plan dated 1601 has survived (Figure 18) and in

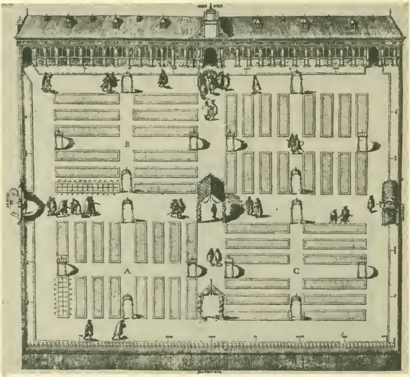


Figure 18. The Hortus Botanicus of the University of Leiden in the time of Clusius.

recent years some ground has been added to the university garden, in which the plantings have been reproduced accurately from those of the original plans. The function of these university gardens then as now was to supply a means of observing and examining plants while they were alive.

When it was impossible to cultivate plants, another method of studying them—i.e., through collections of dried specimens, now known as "herbaria"—was employed, which also dates from the sixteenth century. This method has been exceptionally serviceable for the development of botany. In all probability, Luca Ghini of Bologna (1540) was the first to form a collection of dried medicinal herbs. It is certain, however, that among the oldest known collections of dried plants are those collected by Rauwolf in his travels (1573) and preserved in the state herbarium at Leiden; those of Bauhin (approx. 1600), which are extant in Basle; and those of his pupil Burser (1583–1639), now in Stockholm. These herbaria have deservedly earned their status as major sources of scientific information, and they are on an equal footing at least with the botanic gardens.

It is a remarkable fact that the large-scale expansion of this type of botanic study that took place during the sixteenth century was

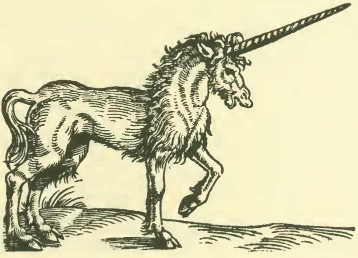


Figure 19. Gesner's unicorn (1551).

not paralleled by anything in zoology. Several zoologists did, however, concentrate their energies on compiling books of animals: the Swiss Konrad Gesner, whose botanical study we mentioned earlier, the Italian Ulysses Aldrovandi (1522–1605) and the Englishman Edward Wotton (1492–1555), whom we list in the order of their importance. But only a small part of their works was based on personal observation. Their descriptions were to a great extent derived from Aristotle and Galen.

Gesner gave us, in his *History of Animals,* a work whose illustrations were of a very high quality (Figures 19–21). They were beautiful woodcuts, of which Dürer's rhinoceros is perhaps the best known, though it is not the most beautiful in every respect. Gesner also set for his other draftsmen and engravers exceedingly high

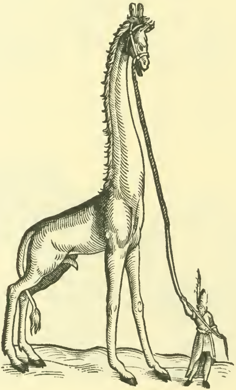


Figure 20. Gesner's giraffe (1551).

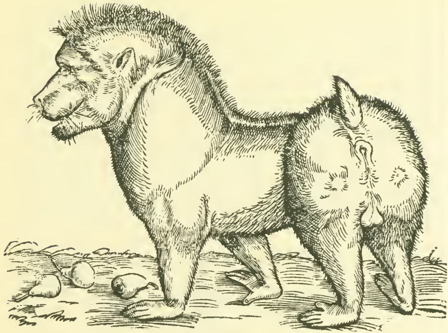


Figure 21. Woodcut of Gesner's hyaena (1551) which served as a model for Johnston's copper plate, Figure 22.

Figure 22. Papio from *Historiae Naturalis Animalium* by Johannes Johnston (Amsterdam, 1657) copied from Gesner's hyaena and appearing reversed when printed.

standards, and lavished the same care on his literary style that he did on his drawings. His language is elegant and eloquent, but his book is lacking entirely in the scientific standards that were evident in the herbals of the period, even if the application of such standards in the herbals was not always successful. According to Gesner, the knowledge of zoology was useful to the physician, the huntsman, and the cook. Also the study of animals, of bird-song, and of the lives of bees and ants, cheer our lives. But scientific research, even accurate descriptions of animals—their exterior structure, etc.—remained to him a matter of secondary importance.

Thus the zoologists of the sixteenth century—even Gesner, the best of the lot—are rightly called the "encyclopaedists." Their writings are encyclopaedias, devoid of any originality.

Gesner's *History of Animals* shows a striking dichotomy. The European animals that he actually saw, both the domesticated and the wild, are beautifully illustrated—the mammals, birds, fish, snakes, worms, and even the insects. On the other hand, the animals indigenous to Asia, Africa, and America—and which seem to have been illustrated from purely verbal descriptions—are pictured in a manner that strikes us as truly hilarious. They add to the gaiety of nations. Moreover, Gesner included the purely mythological creatures—the unicorn, the hippogriff, the cockatrice, and a number of half-human monsters. He was also the first to describe the *jumar,* the bogus hybrid between the horse and the cow. Apparently he wanted to make his four-volume, 3,500-page work complete.

Perhaps we should not judge Gesner too harshly. Essentially he was a classicist, who compiled excellent and valuable dictionaries. Professionally he was a physician and also a Professor of Greek. He died of the plague at the age of 49.

Ulysses Aldrovandi was born at Bologna, and there he became a Professor at the University. Although he had studied law, philosophy, and medicine, he taught pharmacy. He established a famous botanic garden. His monumental natural history was printed in fourteen thick folio volumes; but, even so, many of his writings have never been published.

The third zoologist we have referred to was Edward Wotton, a son of a college porter at Oxford. He became a very famous physician and published *De Differentiis Animalium,* a work which was very well thought of and which did influence Gesner. Wotton's outlook, however, was strictly medieval and Aristotelian.

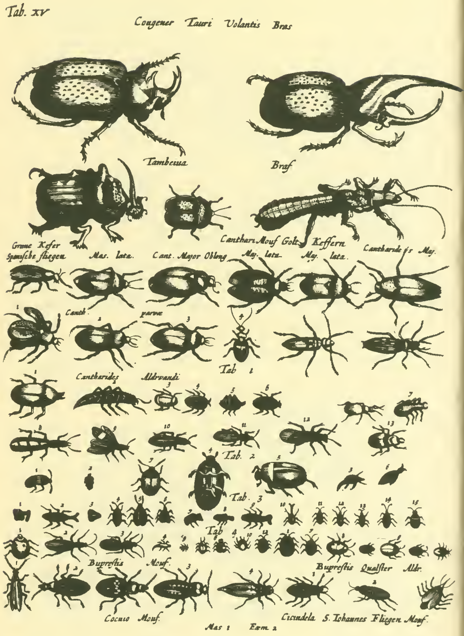


Figure 23. Copper plate illustrations of insects from Johannes Johnston's *Historia Naturalis Animalium*, Amstelodami, 1657.

We can complete our account of sixteenth century zoology by referring briefly to the work of Guillaume Rondelet (1507–1556), who came from Montpellier in France. His work was on fish (*De Piscibus Marinis*) and he included among his fish all the animals that lived in the sea. He made a number of dissections and studied intensively some of the fish, the whales, and the cephalopods. He compared the homologous organs in the different forms and attempted to develop a comparative anatomy. He was not very successful, but he did not hesitate to record discoveries in anatomy that differed from the assumptions of the Aristotelians.

While the four zoologists we have quoted did some work that deserves our respect, the sixteenth century would have been a rather sterile epoch in the history of the science were it not for one man who possessed a pioneering spirit and a revolutionary influence: Andreas Vesalius (1514–1564), who was born in Brussels. He was no zoologist—merely a physician—but his new methods, his independent character, and his bold opposition to the all-powerful idolatry of Galen and to the rigid dogmatism of the medical profession, won for him a place of honor in science.

As a mystic and popular orator the all-conquering and combative Paracelsus had set himself the task of fighting Galenism from the outside, but it was beyond his power to reform the intellectual standards of the physicians. He attacked much of what was useless, but he was unable to construct anything of lasting value. Unlike Paracelsus, Vesalius realized that the unshakable opinions of the Galenists must be attacked from within, and that only a truthful demonstration of their errors could achieve any lasting improvement. On being appointed to a chair at Padua (1537), at the age of 23, he accepted this task and, full of ambition, brought it to a successful conclusion. His impressive work, *De Humani Corporis Fabrica Libri Septem* ("Seven Books on the Structure of the Human Body," 1543) is of imperishable value to anatomy, though of course it was not infallible. It shows a sane, strong, and clear attitude. His portrait (Figure 24) represents him as we know him from his works—a sturdy, strong Fleming, purposeful, matter-of-fact and well balanced, wholly devoid of vague mysticism, no demagogue but a sincere and truth-seeking reformer.

Vesalius was well equipped for his task. As a youth he had dissected as many different kinds of animals as he could find. At the age of eighteen he went to Paris to study medicine. But there the

Figure 24. Andreas Vesalius (1514–1564).

school was dominated by Jacobus Sylvius (Jacques Du Bois, 1478–1555) who later became his bitter enemy. The fissure of Sylvius in the brain, however, is not named for this Sylvius, but for Franciscus Sylvius (Frans De le Boë, 1614–1677) a very able professor of medicine at Leiden. In Paris, Vesalius continued his dissections and soon became so skillful and so well known that he was in constant demand for the dissection of human cadavers in place of the usual barber-surgeons. He collected bones from places of execution and later assembled from them a completed skeleton. He left Paris, however, before he completed his education.

Anatomical drawings were no novelty in Vesalius' time. Artists of genius such as Leonardo da Vinci and Michelangelo have left us some which still reduce to a state of rapture even the spoilt man of

the twentieth century. Leonardo was the first to plan a textbook of anatomy for painters and sculptors. But the Italian artist was not interested in pure science; his tendency was synthetic rather than analytical.

The young Vesalius, who presented his momentous work to the world at the age of 29, understood that the illustrations were to be one of the most important factors in his book. He realized, moreover, that the Italian artists by whom he was surrounded could not help him. He looked around for other help and found it in one of Titian's best pupils, the Fleming Jan Stephan Van Calcar. Through their collaboration and complete mutual understanding, the *Fabrica* came into existence and became at once a monument of a newly awakened science. The year 1543, in which his *Fabrica* was published, also saw the publication of *De Revolutionibus Orbium Coelestium* of Nicolaus Copernicus. In this year the Renaissance had come of age.

Vesalius did not find the way prepared for him when he started on his works. Material for anatomical study was not readily obtained, and his own celebrated teachers in Paris, Du Bois (Sylvius) and Günther, soon looked upon him as an undesirable rebel. He left Paris and returned home, but soon afterwards resumed his pursuits at the University of Padua. Five years of hard work in collaboration with Calcar followed. During these five years he created his *magnum opus*.

Before his aim could be achieved, violent opposition and numerous obstacles had to be overcome. At the time of his appointment, the dissection of bodies was practiced occasionally but its sole purpose was to demonstrate graphically the truth of Galen's statements and not to check the accuracy of what Galen had written. No professor would ever soil his hands with a scalpel. The manual work was the duty of surgeons, and the professor, seated aloft, only gave instructions.

Vesalius thought he could prove that Galen's descriptions were not derived from the examination of human bodies, but from those of such animals as monkeys, sheep, and pigs. Did he not frequently notice fundamental deviations from Galen's so-called "facts"? The official Galenic doctrine had its defence prepared: the facts referred to by Vesalius were undeniable but they were exceptions—or perhaps the human body had changed since Galen's day. "Galenus dixit" ("Galen has spoken") was the sole permissible dictum.

Vesalius triumphed—ultimately. Meanwhile he was subjected to the most vicious attacks, and even his personal character was assailed. He resigned his chair at Padua and became the personal physician of the Emperor, Charles V. When Charles was succeeded by his son, Philip II, Vesalius continued to serve as the court physician, but he resigned his post in 1564 and journeyed to Venice. He travelled from Venice to the Holy Land but, supposedly, was drowned on his return trip. There is a tradition that he died on the island of Zanto and is buried there.

Vesalius' chief opponent, Sylvius, deserves some attention. Sylvius was a student of the classical languages, Latin, Greek, and Hebrew. He also wrote an excellent French grammar. He was nearly fifty, however, when he started to study medicine, and his orientation toward the past had long been firmly set. Galen, he thought, was infallible, and his work on human anatomy was authoritative and complete. Thus any structures in the body of man that differed from Galen's descriptions must have changed since the time of Galen. Sylvius accepted the universally held belief in the inheritance of acquired characters and so had a ready-made explanation for all these supposed changes. Locy in his *Growth of Biology* cites the following rather humorous incident (p. 174):

> . . . he [Sylvius] asserted that the straight thigh bones which, as everyone saw, were not curved in accordance with the teaching of Galen, were the result of the narrow trousers of his contemporaries, and that they must have been curved in their natural condition, when uninterfered with by art.

But Vesalius did win the day; he soon established his own school of thought. A number of his younger contemporaries followed in his steps. Each of them dealt with a small part of the human body, but in greater detail than Vesalius himself had been able to do. Bartolomeus Eustachi (1520–1574) examined the sensory organs, especially those of hearing; Realdo Colombo (1522–1559) studied the organs of the pulmonary blood circulatory system; Gabriele Fallopio (1523–1563) described the sexual organs, discovered the eardrum, and traced the course of certain important nerves; and Fabricius ab Aquapendente (1537–1613) chose as his field the vascular system and embryology. They saw to it that Vesalius' work was continued, and they improved on his observations. Their research did not lessen Vesalius' glory in any way, but rather served to increase it.

Vesalius was the pioneer whose destiny it was to unify his science.

There had been only one man before him who might have accomplished this task—Leonardo Da Vinci, who perhaps could have done it on even a wider scale. But Leonardo had too many other interests.

Leonardo, it is true, had the talents and the opportunity to lay the foundation of comparative anatomy, but this duty was taken on by others, primarily Volcker Koyter (Volcherus Coeiter, 1534–1574), who was born in Groningen. He became a military doctor in the French army and later a municipal physician in Nuremberg. He had been a pupil of Fallopio, Eustachi, and Aldrovandi. His particular interest was the skeleton, and he developed the knowledge thereof in two directions: he compared the human skeleton with those of vertebrate animals, and with the stages in the development of embryos and young children. From this beginning he continued his researches in several other directions. He was the first to study the growth of the chicken in the egg, and he also investigated the soft parts of the animal body. Koyter, like his contemporary anatomists, had not had any philosophical schooling. The compilation of facts satisfied him completely. Any comparison between the different groups of animals, which he had examined, lay beyond the scope of his vision. His primary objective was, and ever remained, human anatomy. His was an important contribution to the foundations of comparative anatomy, later to be laid by Camper and others.

The same may be said of Fabricius ab Aquapendente (1537–1613), who approached the subject of comparative anatomy from quite a different angle. For him the most important thing was the function of each organ. In his work, the comparative anatomy of animals and embryology were of only secondary importance. In a number of animals he tried to investigate a specific function (locomotion, speech, sight) by establishing similarities between the organs performing these functions. He had a vague idea of what was later to become physiology, but his physiology was strongly Aristotelian and was built, moreover, purely on observation and not on experimentation.

The development of physiology was following that of anatomy. Indeed anatomy without physiology would be rather meaningless —a mere collection of facts. Thus any anatomist who had described the structure of an organ correctly would add greatly to the significance of his work if he could discover what the organ did. During this period in the development of zoology, anatomy and physiology were so intimately connected that neither could progress very far

without the other. The great physiological discovery that followed the anatomical advances of Vesalius and his pupils was that of the circulation of the blood. But first had to come the partial discovery, that of the pulmonary circulation!

As we have noted earlier, our first record of this discovery dates from the thirteenth century, the record left us by Ibn-an-Nafis. But now, in the European Renaissance, the pulmonary circulation was described by three anatomists—Servetus, Columbo, and Cesalpino—and it may have been discovered independently by two of them.

Miguel Servet y Reves (1509–1553) is known to science and to martyrology as Michael Servetus. He was a Spaniard and very restless. He traveled into Italy and then into Germany, where he published *De Trinitatis Erroribus*. Because of this book, he found it expedient to leave Germany speedily and to change his name. He studied medicine at Paris and practiced anatomy with Vesalius. Basically he was a mystic and a theologian. He became a Unitarian and, as a Unitarian, he was unwelcome in the Catholic countries. He sought refuge at Geneva with John Calvin, the leading Protestant. His *Christianismi Restitutio*, however, written earlier when he was at Lyons, made Calvin his enemy and he was burned at the stake in Geneva on October 27, 1553.

Paradoxically, it was his mystical belief in the blood (the blood is the life) that led to his discovery of the pulmonary circulation. Blood, he observed, passed from the right chamber of the heart to the lungs, where it was inspired by the respiration, and purged of the bodily impurities. It returned then to the left chamber. The blood, he stated, does not pass through the wall of the heart.

Vesalius' pupil Columbo, whom we have mentioned earlier, published his discovery of this lesser circulation in 1559, six years after the death of Servetus. He claimed that the discovery was his own. His pupil, Cesalpino, whose botanical work we have discussed, also described this circulation, and in this connection he used the word "circulation" for the first time. Columbo was the only one of his predecessors who was cited by Harvey.

William Harvey (1578–1657) was not merely an anatomist who dissected and observed. He was an experimenter par excellence. He was born in England, educated at Cambridge and, later, at Padua, where he was a student of Fabricius. He became a famous physician in London and, ultimately, the court physician to King James I. On the death of James he became the personal physician

to Charles I. When the latter had to flee from London to Oxford because of the imminence of civil war, Harvey accompanied him. By this time his great book, *De Motu Cordis* (1628), had been published in Germany. His second major contribution was to embryology. This was his *Exercitationes de Generatione Animalium,* which was not published until 1651—after he had fled from London.

Harvey, the great physiological experimenter, was a pupil of a pupil of a pupil of Vesalius who, much earlier, had seen the necessity of identifying the function of the organs exposed by dissection. The *Fabrica* itself stimulated research in this direction. The nineteenth chapter of the seventh book—the final chapter—contained various directions for physiological research:

Hence the students rightly begin to examine the action and function of the part, by practicing on corpses, so that they may shortly afterwards set to work on the living animal. Since, too, the parts of the body are manifold and destined to perform various functions, there can be no doubt that the vivisections must be numerous.

When Vesalius went on to explain how one may examine the functions of ligaments and tendons, of muscles and nerves, of the spinal cord, of the veins and arteries of the heart, of the digestive and reproductive organs, and of the brain, it becomes evident that the vision of this genius extended beyond the scope of anatomy and, although we may deplore the fact that he did not work along these lines himself, we have no right to reproach him. He had achieved enough. His pupils continued to work in the field of anatomy but, after Vesalius, physiology remained almost untouched. Still, an occasional anatomist such as Fabricius did show an interest in the functions of organs. Fabricius, moreover, was to see a major advance in physiology made by one of his own pupils.

Harvey carried out a simple experiment: the binding of the arm above the elbow, with a piece of string as his sole instrument. But Harvey had other aids to his piece of string: his keen observation, his accurate calculation, and his strictly logical reasoning. In this way he was able to determine the main facts about the *circulation* of the blood, to oppose his theory to the ephemeral and unfounded notions of Aristotle and Galen, and to lay the foundation of modern physiology.

The circulation had been explained by Aristotle in this way: the heart sucks up blood and adds to it the "pneuma," which enables the limbs to move and the brain to think. Subsequently the heart

distributes it out to the other parts of the body. Galen's theory differed from Aristotle's in that he considered the intestine as the place where the blood was extracted from the nutritive substances; thence, he thought, it was brought to the liver where it was "boiled" again, and that this relatively pure blood went to the right ventricle of the heart through the veins, all of which emanate from the liver. Here in the heart the blood supposedly underwent a final process of purification, after which the residual products were removed through the lungs. Next, the blood flowed through the apertures in the partitional wall of the heart, into the left ventricle and then to the body, where it coagulated to muscle and, in this process, was used up. The heart produced the vital heat by which the blood was boiled; the air from the lungs tempered the excessive heat of the heart. Respiration and heart-beat therefore were two manifestations of temperature regulation; the difference in their rhythm was irrelevant. During its expansion the heart sucks up blood, because the blood flowed away when the heart contracts. Therefore expansion is the active, contraction the passive operation.

In opposition to this Aristotelian-Galenistic theory, Harvey now formulated his own: (1) the heart expands passively and contracts actively; (2) the auricles of the heart contract before the ventricles; (3) the contraction of the auricle forces the blood into the ventricle; (4) the arteries have no power of pulsation—they dilate passively as a result of the blood flowing in them; (5) the heart is the pump for the blood; (6) in its journey from the right ventricle to the left auricle the blood passes through the lungs; (7) the quantity of blood and the speed of movement outside the heart make it essential that the greater part of the blood return to the heart; and (8) the blood returns to the heart through the veins.

Only one of these postulates, namely the sixth—concerning the propulsion of the blood from the right ventricle to the left auricle through the lungs—can be traced to the discovery of the lesser circulation by Ibn-an-Nafis, Servetus, and Columbo. But all the others, so strongly opposed to the doctrine of Galen, are the work of Harvey. Harvey's power of driving home his point of view so rapidly and so successfully is a characteristic which we should not have expected to find in him. Though he is depicted as an irascible, impulsive figure, burning with indignation at the scientific shortcomings of Galen's followers, he knew the value of patience. In 1616 he demonstrated his findings in lectures and discourses, but he waited twelve

years before giving to the world (in 1628) his *Exercitatic Anatomica de Motu Cordis et Sanguinis in Animalibus* ("Anatomic Research into the Movement of the Heart and the Blood in Animals").

In this he presents us with an account of the nature of the circulation which would be regarded as correct today and in which only one link was missing: the connection between the arterial and the venous blood streams. This he explained in the following way: the blood filters through the tissues and thus passes from the arteries into the veins. It remained for others who had magnifying glasses at their disposal—Leeuwenhoek and Malpighi—to solve this final detail of the circulation, because they were able to perceive that capillaries act as intermediaries.

By this discovery alone Harvey earned a central place in biology. To be looked upon as the founder of a science as comprehensive as physiology, is more than enough in itself to earn the title of pioneer. But in other fields too he rendered original services: for example, following the footsteps of his teacher Fabricius he studied the development of vertebrate animals. It may at first sight seem odd that, in addition to his experimental work on the circulation, he should choose to include in his field of study something as seemingly remote as embryology. This is explained, however, by the theories about the central significance of the heart which were generally held in Harvey's time.

During the Renaissance, three basic embryological theories were prevalent: (1) the Aristotelian, which on philosophical grounds looked on the heart as the essence of life and held it to be the first-formed organ; (2) the Galenic, which held that the liver was first formed; and (3) the opinion of the Arabian physicians, according to whom the organs all originate from three primary vesicles of the embryo. Of these three theories, that of Aristotle was held in the highest repute toward the end of the Renaissance. The only means of testing and proving which theory was correct was that of accurate observation.

Hippocrates had already pointed out that if one causes twenty or more eggs to be hatched by a few hens, taking away and carefully breaking open one egg a day from the second to the last, then it must be possible to give an accurate description of the development of the chick. Yet neither Hippocrates himself nor any other naturalist in the 2000 years following him put this obvious method into practice. The encyclopaedist Aldrovandi started on it and came to

the conclusion that Aristotle was absolutely right, as he did see the heart being formed first. That is as far as he went.

His pupil Koyter continued this work. In a short, clear, objective, and purely pragmatic treatise (1573), he gave many further details: the heart was formed on the third day; the brains on the fifth; the convolutions of the brain were perceptible on the tenth; etc. He was followed by Fabricius ab Aquapendente, whose services in this field lie chiefly in the illustrated atlas (1621) with which he supplemented his findings. His conclusions, however, were rather vague and far-fetched in content and in treatment. The illustrations in this atlas (the oldest after Koyter's) depict various stages in the development of the embryos of chicks, snakes, rats, pigs, sheep, and cattle.

Next came Harvey with his unillustrated treatise *Exercitationes de Generatione Animalium* ("Research into the Development of Animals," 1651). His conclusions are those of a keen observer and logical thinker, free from any philosophical prejudices, but limited in that he sees only that which is accessible to the naked eye. His conception of the "egg" is more or less abstract; the egg is not a concrete object. ("In fact we will call every beginning capable of life 'egg'.") He thought that there was a stage in the development of all animals during which they were undifferentiated living masses and that this stage constituted the egg. In birds, these eggs were most clearly perceptible; in mammals, they constituted the first stage of the embryonic development; in insects, the larvae changed from an "incomplete egg" to a "complete egg," the *pupae*.

Herein lies one of the major contributions in his work, no matter how primitive his definition of the egg may have been. He summed up his conception in the phrase "ex ovo omnia" and symbolized it in the title page to his work, on which we see Jupiter holding a circular box from which men, deer, birds, and insects emerge (Figure 25). This conception put an end to much uncertainty and speculation as to the origin of the individual animal.

Beside this, Harvey gave a second theoretical exposition. There were two possibilities: either the whole material pre-existed in the egg and was subsequently transformed into the body much as a sculptor produces a statue from a lump of stone, or the mass was generated and shaped immediately on generation. The first possibility he called "metamorphosis," the second "epigenesis." Metamorphosis occurred in the case of insects, which as larva, pupa, and full-grown animal passed through clearly perceptible and well-de-

Figure 25. Title page of Harvey's *Exercitationes de Generatione Animalium* (second edition, 1662).

fined stages of development—during which all the organs simultaneously underwent a change in structure and nature. Here the potentialities of the pre-existing material determined the development to a far greater extent than did the exterior influences.

The development of higher animals such as birds and mammals, however, was by epigenesis: new parts were formed in addition to the existing ones—not all the parts at once, but in a fixed order, determined by the heart, which was the first organ that could be perceived. The development of plants (beans and acorns), was similar: the bud was the source of the future plant and determined the whole of the future form of the additional substances.

Thus Harvey's theory of generation has in it the germ of an antithesis which was later, in the period after Swammerdam and Leeuwenhoek, to lead to violent controversy: the struggle between preformation and epigenesis.

8

The Sun Breaks Through the Clouds

Hesitantly, the day seemed to break. Time seemed to slow down. The dawn of the sixteenth century remained, but it was unable to drive away the darkness all at once. In Europe there was a noticeable decline of interest in the description of unknown animals and plants. What interest there was shifted to other places, such as the East Indies, where, a century later, H. A. van Reede Tot Drakestein (1637–1691), the Governor of Malabar, collected material for the twelve volumes of his *Hortus Indicus Malabaricus* (1678–1703), and where the "blind visionary of Ambon," the "merchant" G. E. Rumphius ("Rumpf," 1628–1702; Figure 26) compiled his manuscripts with undaunted energy. Rumphius succeeded in spite of serious physical handicaps, in spite of disastrous adversity, and in spite of secret and public opposition. Some of these manuscripts, such as the *Ambonese Book of Animals*, were destined to be lost or were not published until after their author's death, as for example the *Ambonese Curiosity-chest* (1705) and the six volumes of the *Ambonese Herbal* (*Herbarium Amboinense*) (1741–1750).

In Europe, the mixed medical and biological sciences were beginning a period of decline—a period, moreover, in which biology was still not strong enough to stand alone. It seemed almost as if the growth of biology had ceased—as if biology was too timid to assert itself. Between the time when the writers of the herbals and

Figure 26. Georg Everhard Rumphius (Rumpf).

the anatomists had flourished, and the full glare of the coming day, lay a period of transition—an interval of more than a century—in which Harvey was the only enlightening personality to emerge.

Then, suddenly, the sun broke through. In a span of fifteen years, three countries produced a total of seven naturalists who, through their strenuous efforts and their achievements, would dominate the biology of the seventeenth century. These seven were the Italians Redi (1626–1697) and Malpighi (1628–1694); the Englishmen Ray (1627–1705), Hooke (1635–1703), and Grew (1641–1712); and the Dutchmen Leeuwenhoek (1632–1723) and Swammerdam (1637–

1683). Later in the century, a German, Camerarius (1665–1721) appeared, a solitary figure who labored during another period of biological stagnation.

Meanwhile, the awakened European intellect continued to evaluate and to explore its increasing fund of information, seeking to fit the newer discoveries into the older knowledge. As we know, facts must be organized if they are to be meaningful, and the organization must also be of such a nature that it can accommodate everything that is known—i.e., if it is to serve as a philosophical system. The classical philosophies were rapidly becoming inadequate. They were unable to incorporate or even keep pace with much of the newer learning.

Thus the seventeenth century philosophers found themselves forced to devise new systems, but they devised them reluctantly and very slowly. The older philosophies, in spite of their inadequacies, continued to show great vitality. They continued to exist and to compete with the new. They were protected by what the human species has always had in superabundance, a massive intellectual inertia.

Often we find this inertia exasperating, but it has its value. We owe to it whatever stability we have. New philosophies always have to struggle for their existence with the old, and this struggle eliminates many of the bright new notions that appear to have been almost insane when we view them in retrospect. The successful innovations, however, weighed in the balance and found not wanting, supply a framework on which science can advance. And there were plenty of these innovations in the seventeenth century. The great progress that biology made later in the century was due both to the newer intellectual orientation and to the technical discoveries made in other sciences.

Despite the great achievement of Harvey, biology seemed to have been resting. In other fields of learning, however, spectacular progress was made. Biology, in spite of its quiescence, did not deteriorate. Scientific advances in other fields gave the biologists a vastly superior technical equipment. The development of the microscope, for example, enabled the biologists to make spectacular discoveries. Even the newer philosophical orientation stimulated the growth of biology, since it showed beyond any possible doubt how important it was for our species to increase its store of biological knowledge.

Let us turn our attention for a moment to perhaps the greatest

re-orientation our species has ever experienced—to our demotion from being the *raison d'être* of the material universe—from being the dominant species that lived in its exact center—to being merely the inhabitants on the surface of one of the minor planets, beset with doubts as to our cosmic importance. To follow this re-orientation we shall have to return to the still unappreciated contribution of Nicolaus Cusanus (1401–1464). Cusa was a churchman, a bishop, and, ultimately, a highly respected cardinal. In spite of his many ecclesiastical duties, however, he found time to indulge in some basic philosophical speculation. He discarded the spherical and closed universe of Aristotle and demonstrated logically that the universe had to be infinite and open. Thus it could not be assigned any definite size or shape: hence it could have no center. He held that the human species—even the earth itself—only appears to be at the center of things. The position of the observer who must always be at the center of his own observations determines, of course, what the universe looks like to him, and this means that the observer himself cannot be ignored in the description of what he sees. Whereas Aristotle had been an absolutist, Cusa was a relativist. None of his learned colleagues recognized the truly revolutionary nature of his speculations.

The publication of the Copernican theory in 1543 made the ultimate triumph of the open universe inevitable. The cosmos was beginning to show some of the aspects of a machine. The ancient superstition we call "astrology" had connected the planets and the heavens with the human body and with our human fate. All during the Middle Ages, the macrocosm and the microcosm had been intimately joined. Here, oddly enough, a cosmic superstition—and to us a very stupid superstition—helped to promote a mechanistic orientation in biology. If the universe were a machine could not our bodies be also mechanical? Some of the seventeenth century philosophers answered this question affirmatively. But the highly honored philosopher, Francis Bacon, did not belong to their school.

Although Francis Bacon (1561–1626) wrote *Sylva Sylvarium, or A Natural History*, 1627, he made no direct contribution of any value to biology. His importance to science and to philosophy lay in his attempt to remove subjectivity from scientific thinking and to his recognition of the fallacies in so much of our human rationalization. Every biologist who values his intellectual honesty should be familiar with Bacon's *Idols of the Cave*, of the *Tribe*, of the *Theater*,

and of the *Market Place*. It is true that he emphasized the importance of experimental investigation but there is no evidence that he himself ever experimented or that he even knew what an experiment, in the modern sense, was.

The level of Bacon's biological sophistication can be illustrated by citing a few of the then current beliefs he endorsed. He believed in "degeneration"—in the ability of one species to change suddenly into another. He also accepted spontaneous generation and thought that different species of animals could interbreed and produce hybrids almost without limit. He published fanciful directives for producing sectorial chimeras in plants. He held that the Ethiopians became black by living in a hot and moist climate and that the thickness of their lips was due to the excessive moisture they retained in their bodies. His statements on biological subjects, as illustrated by the above, are almost uniformly trivial. The advances in technology at this time did far more than the philosophers to advance science.

It was at the very beginning of this century—at some time between 1590 and 1608—that lenses were first mounted in the two ends of a tube. This instrument was the telescope and, when Galileo Galilei (1564–1642) examined the stars and planets with the very superior instrument that he himself had constructed, he made new discoveries wherever he looked. His *telescope,* a plano-convex objective and a plano-concave eye-piece, could also be made in such a way as to have a very short focal range; that is, it could have an objective that has to be very close to the thing examined. This instrument is the compound microscope—although at first, it was a very crude one. In fact, during most of the century, the single but accurately ground lenses of Leeuwenhoek gave more accurate resolutions than any of the compound microscopes. The latter, however, are perhaps the greatest of all technical aids for the study of biology. As we shall see, the great discoveries that came later in the century were made with the aid of the microscope. They were made, however, within the milieu of seventeenth-century philosophy.

One of the most famous of the philosophers was René Descartes (1596–1650), who attacked the problem of the body-soul relationship. He was also a mathematician and an engineer and, to him, the animal body was a machine. Blood flowed through the veins and arteries by the action of a pump, the muscles worked the limbs according to the principles of the lever. And the bodies of animals, he held, were only machines. But to him human beings, were more

than machines in that they had immortal souls; and here he announced clearly a dualistic hypothesis that still persists. To Descartes, however, the soul was an immortal substance and not an integral part of the body. It made contact with the body by way of the pineal gland—the so-called "third eye." It was through the *glandula pinealis*, he held, that the body and soul interacted.

Descartes' attempt to explain the mechanics of the central nervous system illustrates both the universal ignorance of physiology and the unchecked speculations of the times. He believed that the nerves were hollow and had valves at appropriate spots. They transmitted a very subtle vapor which, when discharged into the muscle fibers, caused them to swell in width and to contract in length. It was from such beginnings as these that the science of neuro-physiology developed.

Other philosophers were somewhat more realistic. Thomas Hobbes (1588–1679) held that mental impressions were merely "motions" of the central nervous system. This was obviously not a major contribution. In his *Leviathan* (1651), however, he did make a contribution, for here he envisioned clearly the struggle for existence. He recognized unambiguously that human society was a device that enabled one highly developed animal (man) to become gregarious and to enjoy the benefits of gregariousness. According to Hobbes, ethics itself evolved as a consequence of an animal's finding it expedient to live in groups. Gregariousness, as we know today, has survival value. (From Pt. I, chap. 13):

> To this war of every man, against every man, this also is consequent; that nothing can be unjust. The notions of right and wrong, justice and injustice have their no place. Where there is no common power, there is no law; where no law, no injustice. Force, and fraud, are in war the two cardinal virtues. Justice, and injustice are none of the faculties neither of the body, nor the mind. If they were, they might be in a man that were alone in the world, as well as his senses, and passions. They are qualities, that relate to men in society, not in solitude.

Hobbes' conclusions are realistic and to the point:

> And consequently it is a precept, or general rule of reason, that every man ought to endeavor peace, as far as he has hope of obtaining it; and when he cannot obtain it, that he may seek, and use, all helps, and advantages of war. The first branch of which rule, containeth the first, and fundamental law of nature; which is, to seek peace, and follow it. The second, the sum of the right of nature; which is, by all means we can, to defend ourselves.

We can dismiss other philosophers of the seventeenth century with little ceremony. Pierre Gassendi (1592–1655), an opponent of Descartes, sought to revive the atomic theory of Lucretius but, as he was a priest, he had to admit the existence of a soul and to reject the Copernican universe. Baruch Spinoza (1632–1677) of Amsterdam, who made his living grinding lenses and who, incidentally, is one of the most appealing figures in all history, held that the cosmos has many aspects but that we can recognize only two of them. These are (1) the material, and (2) the spiritual. Here again is the mechanism-vitalism duality.

Gottfried Wilhelm Leibnitz (1646–1716), co-developer with Newton of the calculus, and for years almost the cultural dictator of Europe, conceived of a particulate, ideal substance as the basis of the universe. In our terminology we might define these particles —Leibnitz's "monads"—as units of different kinds of force or energy. The recent discovery of "animalcules," made visible by the microscope, led him to assign a very low order of monads to inanimate substances. These monads exist, he believed, but only as in a dreamless sleep. The monads of plants he held live but are not percipient; the monads of animals are percipient but unconscious. The human soul is a monad that is both percipient and conscious; and God is the Supreme Monad.

We can dismiss very easily such speculation as the above. The fact is, however, that Leibnitz was perhaps the most influential philosopher and mathematician of his age and that his speculations did influence the biologists who followed him.

Although biology and medicine were in the process of becoming separate disciplines, the physicians were still making discoveries in what we should call "pure biology." In many respects the most remarkable of these doctors of medicine was Jean Baptiste van Helmont (1577–1644), who was born in Brussels. His family was both noble and wealthy. Moreover, he married a rich wife. He practiced medicine, but only as a charity. He was a mystic and very religious. He was also an alchemist, and sought inspiration by means of self-hypnosis. He believed in spontaneous generation, a fact we shall have occasion to refer to later.

Van Helmont was influenced greatly by Paracelsus and, like Paracelsus, he set out to refute the ancients. His chief objection to the ancients, however, was that they were pagans. It seemed that neither Aristotle nor Galen was a good Christian such as van Hel-

mont himself was. He made many errors, especially in his interpretations of the experiments he performed; but in spite of himself he did achieve some important results.

Van Helmont investigated fermentation intensively and discovered that the air given off by alcoholic fermentation was the same as that produced by burning charcoal. But he believed that plants obtained their substance from water. To prove this he planted a willow weighing 5 pounds in a tub that contained 200 pounds of dry earth. He watered the tree with rainwater and, after five years, found that the tree weighed 164 pounds but that the soil had lost only 3 ounces. This proved, he held, that the substance of the tree had come from the water.

Now here is an oddity! Van Helmont was the man who coined the word "gas" and who discovered carbon dioxide. He found, moreover, that carbon dioxide was generated from plant products. The question that would suggest itself to a modern biologist would be, "How did carbon dioxide get into the plant?" Yet Van Helmont held that the increase in the weight of his willow tree came from the water it had absorbed. This interpretation gives us an extreme example of an opportunity missed, but the experiment on which it was based was well designed. It was the first one of its kind, as it was based on quantitative measurements. Later workers were to find it important.

Other doctors of medicine added bits here and there to the knowledge of biology. Thomas Bartholin (1616–1680), a member of the famous Danish family of scientists, discovered the lymphatic system. Olof Rudbeck (1630–1702), a Swede, claimed to have made the same discovery. Incidentally Rudbeck also published a number of papers on the native and exotic plants grown in the region around Uppsala. Giovanni Alfonso Borelli (1608–1679) sought to apply mechanical principles to animal movements, and he discovered that the contraction of a muscle was due to the contraction of the individual fibers and not to a change in shape of the whole muscle.

Claude Perrault (1613–1688) studied mathematics and chemistry. He practiced medicine, but his interests changed and he became a well-known architect. A volume of his famous work, *Essais de la Physique* is entitled *Mechaniques des Animaux* (1680). In this he endeavored to place the whole animal body on a mechanical basis. He did not deny the existence of the soul—he merely put it in the background.

Another physician, Nils Steensen (1638–1686), a Dane, studied the glandular system and made a number of anatomical discoveries. He also investigated fossils and almost established a science of paleontology. But he dropped the work. He was converted to Catholicism, became a bishop, and concentrated his efforts on securing more converts to his new faith.

These and other practitioners of medicine continued to add to the factual knowledge of biology. Later in the century, however, biologists such as the seven cited at the beginning of this chapter succeeded in bringing about a revival of the science—an awakening wherein biology was cut adrift from medicine. We might almost describe this revival as a changeover from mere knowledge to organized science. These men, especially the seven named, were bold enough to assign to biology the status of a separate and individual science.

They did not ignore the objective accumulation of facts, such as those assembled by the herbalists and anatomists of the preceding century; they sifted them and probed into them with magnifying glasses. But they were also aware of the problems underlying the mere facts that their observations revealed. Harvey, in his works and in his discussions, had already reacted against the exaggerated objectivity of his time, but as a thinker he was still influenced too much by the old Aristotelian standards. These seven seventeenth century scientists recognized the problems that arose in biology and realized their significance. They knew that the accumulation of facts is an essential and useful basis for any science, but they were also aware that the accumulation of facts alone served only to invest biology with the stamp of learning, and that it was the drawing of theoretical conclusions which gave it its scientific character.

Deep and firmly-rooted in the seventeenth century was the dogma of *abiogenesis*—of *generatio spontanea*—which sprung like a rich flower from the writings of the ancient Greek philosophers. A few of these, such as Anaxagoras, had denied it but they had denied it in vain. The dogma was handed down through the Middle Ages and remained unshaken even in the critical mind of Harvey. Van Helmont, who, as we have shown earlier, had rendered valuable services in his physiological experiments, gives us proof of its persistence: he held that not only are our body-lice, bugs, fleas, and worms generated out of ourselves and our excrements, but that also if we stuff a vat full of wheat and enclose it with a dirty shirt, the ferment exud-

ing from the shirt and mingling with the odor of the wheat changes the grains of wheat—still enclosed in their husks—into mice. To him the miracle was that out of this grain, with its husk, do not come minute, sucking, or immature mice but completely adult animals.

The only possible reaction to these and similar fantasies based on "observation" was that of reliable experimentation such as that undertaken by Francesco Redi (1627–1693). In the course of his research into the production of insects (*Esperienze Intorno alla Generazione degl'Insetti,* 1668), Redi had discovered that these animals have two distinct sexes, just as the higher animals have; that they reproduce by copulation; and that the females lay fertilized eggs from which the young are born. This brought Redi into conflict with Harvey's theory, which accepted the old Aristotelian dictum that the "eggs" of all lower animals are formed out of rotting substances, and it is out of this corruption that insects originate.

Redi's experiments were extremely simple: he let meat rot in pots without lids, some of the pots being left entirely open, others covered with parchment or muslin. He saw that flies attracted by the stench of the rotting meat laid eggs on it where they could get to it and that, after a while, larvae and young flies hatched from the eggs. He saw also that no flies were attracted to the parchment-covered pots and that there no eggs, and consequently no larvae, were found. The smell of the meat escaped through the muslin-covered pots, however, and the flies were attracted to them; but in this case they had to lay their eggs on the muslin, since they could not reach the meat. The maggots thus came out of their eggs into the protective muslin, and, of course, were not found on the meat. His conclusion was obvious: flies do not come out of eggs which originate in rotting meat—they only hatch from eggs laid by flies.

Nevertheless, some difficulties remained: intestinal worms were found in man, and for these Redi could find no explanation, unless, in accordance with Aristotelian doctrine, they were created by "pneuma" from human intestinal fluids. His contemporary Antonio Vallisnieri (1661–1730) suggested as an alternative to this assumption that these animals, too, might have hatched from eggs and might also reproduce by means of eggs—but Vallisnieri was unable to prove this by experiment. Moreover, because of the method of reasoning prevalent in his day, he became involved in the Biblical question of why God had created such parasites in Adam's body.

A second difficulty was caused by the discovery of *infusoria* and

bacteria by Leeuwenhoek (1674–1676) which, by their mysterious appearance in all kinds of fluids, further postponed a solution to the problem of spontaneous generation. It is to Redi that we owe the first important blow which undermined the firmly-rooted doctrine of abiogenesis. A century later his compatriot, Lazzaro Spallanzani (1720–1799), brought the problem another step nearer to solution.

The belief that all animals originate from eggs became more and more prevalent as the seventeenth century passed, owing partly to a technical advance which dominated the biology of the century—namely, the construction of the *microscope*. Invented by Galileo, the microscope was developed mainly by the work of two men: Robert Hooke and Anthony van Leeuwenhoek. Since their time the microscope has developed from a simple magnifying glass into a relatively complex system of lenses and mirrors. If we compare the little instrument used by Leeuwenhoek with the eighteenth century microscopes and, especially, with our modern instruments, we are bound to be surprised and feel a sincere admiration at the results these men obtained with their simple instruments.

Neither Hooke nor Leeuwenhoek was a professional biologist. Hooke was an "engineer" and, through his work as "curator of experiments" at the Royal Society of London, he came into contact with all the various types of research for which instruments were needed. He was interested in the potential technique of flying, he improved the construction of clocks, he drew up plans for the rebuilding of London after the great fire of 1668, and he supervised their practical application. He examined the possibilities of weather-forecasting and studied the problem of the magnetism of the earth. The primary aim of his microscopic investigations was to evaluate the improvements he had made in his instrument. The biological phenomena he observed were only a secondary consideration. But this does not alter the fact that he has left us in his *Micrographia* (1665) a work containing a great many biological observations, most of them insignificant but some of fundamental value.

The following examples will help to show this: his "Observatio XLIII, of the Water-insect or Gnat" gives a detailed description with illustrations (Figure 27) of mosquito larvae, which he finds not only in rainwater but also on the banks of ponds and rivers.

It is supposed by some, to deduce its first original form from the putrification of Rain-water, in which, if it have stood any time open to the air, you shall seldom miss, all the summer long, of store of them frisking to and

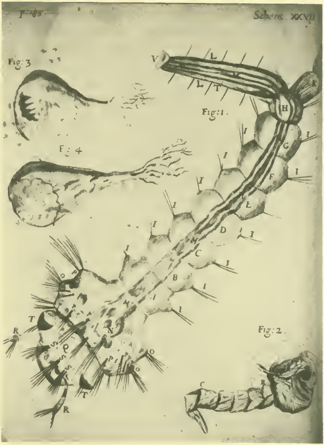


Figure 27. Larva and pupa of a mosquito. (From Hooke, 1667.)

fro. . . . I could perceive, through the transparent shell, while the animal surviv'd, several motions in the head, thorax, and belly. . . . It shows of how great benefit the use of a Microscope may be for the discovery of Nature's course in the operations perform'd in Animal bodies, by which we have the opportunity of observing her through these delicate and pellucid teguments of the bodies of Insects acting according to her usual course and way, undisturbed, whereas, when we endeavor to pry into her secrets by breaking open the doors upon her, and dissecting and mangling creatures whil'st there is life yet within them, we find her indeed at work, but put into such disorder by the violence offer'd, as it may easily be imagin'd, how differing a thing we should find, if we could, as we can with a Microscope, in these smaller creatures, quietly peep in at the windows, without frightening her out of her usual ways. (Hooke, pp. 185–186).

There follows a detailed description. He observed in the living

organism the changes which took place during the transition from larva to gnat. His attention was drawn to an interesting fact:

At length, I saw part of this creature to swim above, and part beneath the surface of the water, below which though it would quickly plunge itself if I by any means frightened it, and presently re-ascend into its former posture; after a little longer expectation. I found that the head and body of a Gnat, began to appear and stand clear above the surface, and by degrees it drew out its legs, first the two foremost, then the other, at length its whole body, perfect and entire appear'd out of the huld (which it left in the water) standing on its legs upon the top of the water, and by degrees it began to move and after flew about the Glass a perfect Gnat. (pp. 187–188).

This transformation from a water insect to an air insect was to him a revelation. It offered him considerable food for thought. Might not one reasonably assume that all organisms, which previously had been supposed to originate in rotting substances, had entered the water from outside, just as the gnats did—probably in the form of eggs? Did the gnats lay these eggs directly in the pool or were they brought there by the rain? Do all water organisms spend a part of their lives in the air? Hooke, who was a technician with little thirst for learning, did not answer these questions, but he did testify to his serious doubts about the dogma of spontaneous generation, and did it some years before Redi carried out his decisive experiments. It may be true, as one of Hooke's biographers said about him, that he started many things but completed few; yet he certainly was one of the foremost research scientists working in the biological field, even if he did not concentrate in biology.

This is also evident from another statement we owe to him and which made him the pioneer of a later completely independent branch of biological study:

I took a good clear piece of Cork, and with a Pen-knife sharpen'd as keen as a Razor, I cut a piece of it off, and thereby left the surface of it exceeding smooth. . . . I, with the same sharp Pen-knife, cut off from the former smooth surface an exceeding thin piece of it, and placing it on a black object Plate, because it was it self a white body, and casting the light on it with a deep plano-convex Glass, I could exceeding plainly perceive it to be all perforated and porous, much like a Honey-comb, but that the pore of it were not regular: yet it was not unlike a Honey-comb in these particulars. First, in that it had very little solid substance, in comparison of the empty cavity that was contained between, as does more manifestly appear by the Figure A and B of the XI. scheme [Figure 28], for the Interstitia, or walls (as I may call them) or partitions of those pores were neer as thin in proportion to their pores, as those thin films of Wax in a Honey-comb (which enclose and constitute the sexangular cells) are to theirs.

Next, in that these pores, or cells, were not very deep, but consisted of a

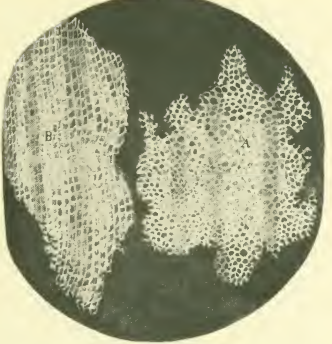


Figure 28. Sections from cork, showing cells. (From Hooke, 1667.)

great many little Boxes, separated out of one continued long pore, by certain Diaphragms, as is visible by the Figure B, which represents a sight of those pores split the long-ways. (pp. 112–113).

Nor is the kind of Texture peculiar to Cork only: for upon examination with my Microscope, I have found that the pith of an Elder, or almost any other Tree, the inner pulp or pith of the Cany hollow stalks of several other Vegetables: as of Fennel, Carrets, Daucus, Bur-docks, Teasels, Fearn, some kinds of Reeds etc. have much such a kind of schematisme, as I have lately shewn that of Cork, save only that here the pores are rang'd the long-ways, or the same ways with the length of the Cane, whereas in Cork they are transverse. (p. 115).

For, in several of those Vegetables, whil'st green, I have with my Microscope, plainly enough discover'd these Cells or Pores fill'd with juices and by degrees sweating them out: as I have also observed in green Wood all those long Microscopical pores which appear in Charcoal perfectly empty of any thing but Air (p. 116).

Thus Hooke discovered the elementary constituent of the plant, the *cell,* which is surrounded by a cell wall, and he supposed that his "cells" served to transport matter. But this description is presented to us casually, interpolated among a mass of other observa-

tions. It might well have passed unnoticed had it not been that soon after its publication, two works appeared almost simultaneously, both contributing to the understanding and the significance of these cells. These books were *The Anatomy of Vegetables Begun* by Nehemiah Grew (1672) and the *Anatome Plantarum* of Marcello Malpighi (1675, with a second part in 1679). Both books contain the results of research started many years before 1667, the year when Hooke's *Micrographia* was published, but they definitely show its influence. In many respects the contributions of Grew and Malpighi run parallel, although there are several noticeable and characteristic points of difference. Grew's book was written in a spirit half philosophical, half accurately scientific; it was lumbering and didactic in style. Malpighi's expositions were broader in conception and more literary. They contained little philosophical reasoning but had an almost mathematical exactitude. They were accurately descriptive and clearly differentiated. In Grew (Figure 29), speculation appeared too early, before the fundamentals had been soundly laid; in Malpighi (Figure 32) there was first a solid foundation and then a comprehensive survey.

It was probably this difference in the way that Malpighi and Grew approached their problems that was responsible for the differences in their popularity. Malpighi's work remained in the original Latin while Grew's work, originally in English (1672–1682), was translated and published in Latin (1678), in French (1675, 1679, 1685, 1691), and in Italian.

When he wished to mark a typical analogy between various plant tissues, Malpighi would stress the cell concept; and he did this more frequently than Hooke, who gave his "cells" a rather perfunctory meaning. Starting from microscopically visible plant organs and tissues, Malpighi systematically tried to discover their constituent parts. Everywhere in the plant-organism he saw cells, which he called "utriculi." In addition to these cells, he observed vessels and fibers, but he did not manage to prove the significance of his "utriculi" as the basic unit in the structure of plants.

Similarly Grew, although he frequently mentions "cells," "bladders," or "vesicles" in his *Anatomy of Plants* (Figure 30), was unable to identify the biological role of his "cells." On some points Grew went into greater detail than Malpighi, especially on the question of the cell itself. He stated that "they contain a fluid which is always transparent and probably always thin and liquid like water."

Figure 29. Nehemiah Grew (1641–1712).

The particular service of Grew and Malpighi, then, lay not in the proof of the significance of the cell as a foundation stone but in the opening up of a completely new field of research, that of *plant anatomy.* Grew's text is somewhat obscured by semi-philosophical explanation, but his illustrations are in no way inferior to Malpighi's; they are in fact more accurate and more detailed. But Malpighi had a wider and less scholastic vision. It is significant that it is in his work that we find the first pictures of abnormalities in plants—of *plant galls,* containing live insect larvae (Figure 34). We shall have to return to the work of Grew when we discuss the discovery of sex in plants.

Figure 30. Title page of a French edition of Grew's *Anatomy of Plants*, 1685.

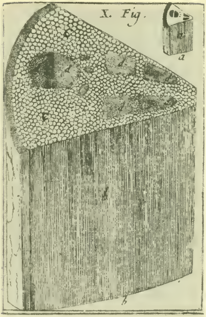


Figure 31. The structure of a stem. (From Grew, 1672.)

This contrast in outlook between the two great men also accounts for another profound difference between them: Grew concentrated all his research on the anatomy of plants, but Malpighi's work was more comprehensive. This is evident from the complete edition of his work, which appeared in Leiden in 1687; two volumes, one (170 pages) containing a reprint of the plant-anatomy, the other (380 pages) containing medico-anatomical and zoological "publications." These volumes were later followed by a volume of *Opera Posthuma* (1697), also of over 300 pages. These included contributions to the study of human anatomy, including the structure of the lungs, the brain, the skin, the tongue, the intestines, and the glands. In addition, they included some purely zoological research—for instance the now-famous studies on the silkworm (1669) and on the development of the chick within the egg (1672). Malpighi demonstrated that in the lungs of a frog, air and blood come into contact but remain separated by a membrane, and that the blood flows back to the heart through the capillaries joining the branches at the ends of the arteries. This was an important improvement on Harvey, one later to be verified, established, and more thoroughly investigated

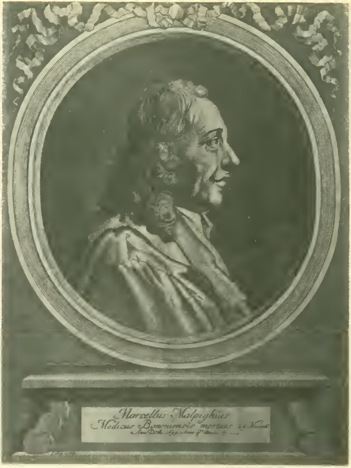


Figure 32. Marcello Malpighi (1628–1694).

by Leeuwenhoek. Malpighi proved that the skin really consists of three layers: the epidermis, the corium, and, between them, a layer of mucous cells which is now called the "layer of Malpighi"; he discovered the presence of papillae on the tongue, and described what came to be known as "Malpighian bodies" in the kidneys and the spleen.

Malpighi, as we have mentioned, has also left us an excellent work on the development of the chick within the egg. Fabricius, Koyter, and Harvey had preceded him in this, but his two short treatises of 22 quarto pages, with twelve splendid illustrations, show how much more deeply he was able to penetrate with his magnify-

Figure 33. Title page from Malpighi's *Anatome Plantarum, pars altera* (London, 1679.)

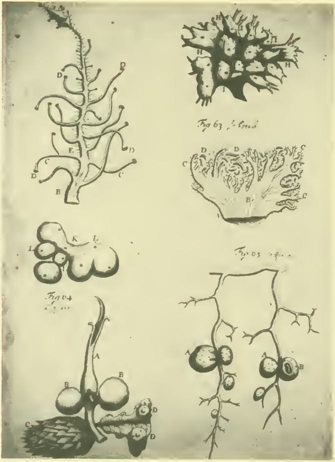


Figure 34. Plant galls. (From Malpighi, 1679.)

ing glasses the development of the young bird. His descriptions and illustrations of such processes as the forming of the brain, the spinal cord, the eyes, and the heart are so accurate, if one considers the limitations of his instruments, that during the following hundred years no one managed to improve upon them.

His greatest achievement, however, was his research into the structure and development of the silkworm. With warm enthusiasm and awe-inspiring energy he abandoned himself to this task. He made heavy demands upon his mind and body and overstrained his eyes, but he must have found a reward for all his sacrifices, since he once said that "so many miracles of nature were revealed to me,

that I experienced an intense inner satisfaction which words cannot describe." He described accurately the silkworm's respiratory system, so different from that of the vertebrate in that it did not have lungs or gills as its central organs, but rather a tubular network ramifying through its whole body. He found this type of respiration in many other animals. He drew the central nervous system as a complex of rope ladders, with the brain lying in a ring at the anterior end of the body. He explained the structure of the digestive organs and the apparatus for the production of silk.

The personality of Malpighi was that of a born research scientist. He was a man who recognized no limits, who tried to probe the secrets of the living organism wherever he sensed their presence. By no means a systematist confining himself to one field, he was deeply convinced of the wisdom later to be formulated by Goethe: "Denn wo Ihr's packt, da ist es interessant" ("Its interest lies wherever you come to grips with it").

This gave him a close spiritual affinity with the Dutchman whom we may justifiably place next to him, Antony van Leeuwenhoek (Figure 35). As far as we know, the two never met, but they were continually in contact, not so much through personal correspondence as through their mutual membership of the Royal Society of London. This was the newly-founded institution which from its inception in 1660 had become almost immediately the international clearing house for the exchange of ideas between natural scientists of different countries.

Leeuwenhoek was a unique personality in the world of learning: he had no university education and no scientific appointment. His small post as usher to the Alderman of Delft made few demands on him, and he was rich enough to lead the life he fancied. He began to specialize in grinding lenses out of glass or quartz, lenses which had a magnifying power of from 40 to 270 diameters. He mounted the lenses as simply as possible, in brass or silver or even in gold slabs with a handle (Figure 36), and with these he studied everything that interested him. He became fascinated by the world of the hitherto invisible, which now danced before his eyes. He was rarely in need of larger, more complicated instruments; if the object to be examined was somewhat intractable, he would build a more elaborate apparatus such as his "instrument for examining eels" (Figure 37), in which he was able to examine the transparent tail of an eel with his lenses.

Figure 35. Antony van Leeuwenhoek (1632–1723).

His burning curiosity drove him to inspect the most varied objects; whatever he saw, he preserved in drawings and described in letters. Diverse as are his objects, his letters are equally varied. Their contents include information of a domestic and homely kind, side by side with information which, from a scientific standpoint, was to be ranked among the most important discoveries of his day. Leeuwenhoek never wrote a book; his lack of scientific training and his rather unsystematic character did not permit him to. But whatever held his momentary interest or fascinated him he set down in

Figure 36. Microscope of Leeuwenhoek in the Netherlands Museum for the History of Science at Leiden.

Figure 37. "Aalkijker" for observing the circulation of the blood in eels. (From the Netherlands Museum for the History of Science.)

a letter to the Royal Society of London, to the Academie des Sciences in Paris, or to "Persons of High Standing and Great Learning" such as Constantyn and Christian Huygens, Leibnitz, the Grand

Pensionary Anthony Heinsius, Nicolas Witsen, the Lord Mayor of the City of Amsterdam, Baron F. A. Van Rhede, the Baron of Renswoude, and many others. Most of these letters were published in the *Transactions* of the Royal Society, and later republished in a Dutch edition. At present a new edition of his letters is in preparation by the Leeuwenhoek Committee of the Royal Netherlands Academy of Science.

Leeuwenhoek's work reached the heights in three directions: (1) he was the first to distinguish between bacteria and other unicellular organisms; (2) he established, for all time, the existence of a connection between the arterial and venous blood-circulation by his discovery of the capillaries, and (3) he made statements about the presence of "tiny mobile animals" (spermatozoa) in the male genital fluid. With this he laid the foundation for some penetrating theoretical discussions.

On the 9th of October, 1676, he addressed a letter to the Royal Society, and on the 7th of November an almost identical one to Constantyn Huygens, containing an account of his experiments with "various" types of water: well water, rainwater, and water (probably well water) which he had left standing two days with ground pepper in it. In the first two "types of water" he discovered an abundance of rotifers and infusoria, but the pepper solution yielded a different picture: organisms of both these groups had been replaced by immeasurably smaller animals, "an incredible multitude in each drop, rapidly increasing in numbers, so that I can honestly say that I saw living and moving therein more than an hundred thousand, in one single drop taken from the surface, whilst others estimate it at ten times this figure." He also discovered a "fourth kind, so small that I am at a loss to describe its shape."

But Leeuwenhoek did not leave it at that: next, he tried to ascertain the influence of air on the formation of bacteria. He filled two long narrow glass tubes with a pepper solution; he left one open at one end and sealed the other. The result was that all kinds of bacteria appeared in the first tube and only a single, rather large type, in the second. But within a few days of opening the latter, all the other types of bacteria were present in it, too. Thus he discovered the difference between what we now call "aerobic" and "anaerobic" bacteria (June 14, 1680).

Leeuwenhoek was fortunate in his choice of a pepper solution for here, in contrast to the other solutions, only *bacteria* appear and, in

particular, the large varieties such as the species *Azotobacter* and *Amylobacter* in addition to the innumerable smaller ones. That he really did see bacteria is also proved by his statement: "Since the smallest animals which I find daily in my infusions have a length three times smaller than the axis of a blood-corpuscle, they must actually be quite 25 times (3^3) as small." This order of magnitude applied only to small bacteria.

Among Leeuwenhoek's scientific friends was Christian Huygens, to whom he showed "the small insects which perpetually move about in the water." While Leeuwenhoek the observer gave little thought to the origin of the bacteria he found in his pepper solutions, Huygens the thinker added in a note to Leeuwenhoek's letter: "I think it probable that they come out of the air, for they are small enough to float in it. Once they are in the water they are able to reproduce in it, as the writer claims he has seen them do." Again the problem of spontaneous generation reared its head, but no decisive solution was found for it as far as bacteria were concerned. This had to wait until Spallanzani's experiments a hundred years later.

Leeuwenhoek also studied accurately the circulation of the blood, known only superficially from Malpighi's work. At first (1683–1689) he reported rather vague observations on frog's feet, tadpoles, the wing-membrane of bats, and rabbits' ears, which amounted to no more than:

These blood-vessels which we call by the names arteries and veins are to be seen in very large numbers at the finger-tips and each curves round so that it is impossible to follow up the individual course of each vessel. All these vessels are so small or narrow that not more than one particle of blood is able to pass through them at a time. When, however, I examined these same fingers near the first or second phalanx, I found that these vessels we call arteries and veins were larger, so much larger, that the blood in these vessels looked red in colour.

Later (1698) he gave a more detailed account:

I placed a tiny eel, the size of my little finger, in a glass tube, the width of a writing quill and placing this under a magnifying glass, I handed it to the artist bidding him pay special attention to the course of the blood in one or two blood-vessels which were very plain to see, telling him that all the vessels which led the flow of blood to the outermost parts, are called arteries (arterial vessels) and that when the blood has reached the finest vessels and flowed back again, that those vessels bear the name of veins (venous vessels), although he could see very clearly that they were one and the same vessels.

The "artist" produced the diagram shown in Figure 38.

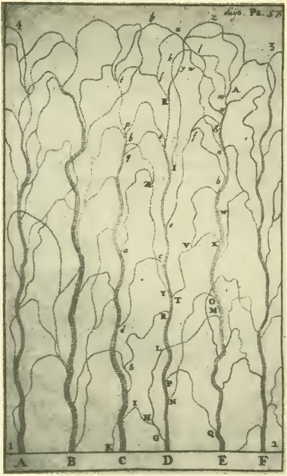


Figure 38. Leeuwenhoek's illustration of capillaries connecting arteries and veins. (From *Opera omnia,* 1715–1722.)

These blood-vessels did not lie in the very tip of the ell's tail, but lay a short way from the tip at the end of the fins. . . . The draughtsman has not indicated all the blood vessels by lines but some by dots, and the dots are supposed to be particles of blood which he saw very clearly flowing through the vessels and which are the same particles that make the blood seem red. . . . All the narrow vessels resembling in fineness H.I.K. or H.L.M. are so slender that I am sure that if one were to divide up the course sand, we call scouring-sand, into ten times an hundred thousand parts, this would still be too coarse to pass through the said vessels. This being so, one may be convinced of the particular fineness of the vessels through which the blood circulated, and if it were otherwise, how would all the parts of the body continually receive nourishment?

Leeuwenhoek has therefore definitely observed the connecting capillaries between the arteries and the veins and established that the red blood corpuscles are propelled from one blood vessel to the next, a fact which for all time, fills a hiatus in Harvey's classic work.

Finally, came the third discovery, which was to consolidate Leeuwenhoek's fame: in November, 1677 he received another visit from one Ham, a student, to whom he had previously shown his microscopes. Ham had also done microscopic work and told him "that he saw living organisms in the male genital fluid, and these animals as this same Gentleman saw, have tails and do not remain alive longer than 24 hours." Leeuwenhoek assured himself with his own eyes that Ham was right; he had previously seen something like this, but at that time "had taken the said animals for corpuscles; and because I felt aversion to examining them again, I left it at that, in those days." But now he perceived the difference between *blood-corpuscles* and *spermatozoa*, for "the animals had a roundish body, somewhat blunt and round in front, at the back somewhat pointed, ending in a long tail, approximately 5 to 6 times as long as the body. It was very transparent and approximately 25 times as thin as the body." Next year he illustrated this description with a drawing of these same animals lying dead, being the first drawing of human spermatozoa (Figure 39).

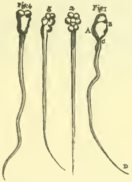


Figure 39. Spermatozoa. (From Leeuwenhoek, 1698.)

After that, things moved fast: he caught the sperm of many different animals and everywhere he found his little animals. He found them also in the vagina of female animals after copulation. This

opened up a completely new vista for him: Harvey had maintained that all living organisms originate in eggs, but now this seemed doubtful. Popular opinion agreed with Harvey:

> I am aware that many learned Gentlemen to whom I have spoken, usually maintained that no male seed enters the womb; for they say it has never been found there and that only a vapour from the male seed penetrates the womb and that this causes fertilization.

Leeuwenhoek's story was received in London with considerable skepticism. Grew, then secretary of the Royal Society, referred him to Harvey's proofs of the role played by the egg in the process of reproduction and to the work of a Dutchman Regnier de Graaf (1641–1673), who thought he had discovered the human egg-cell. The latter

> audaciously declares for certain that the seed of a man is nothing but a vehicle for a certain Sal Volatile or animal spirit, causing conception, i.e. introducing into the egg of the Female, the living sensation (Contactum Vitalem).

But Leeuwenhoek stuck to his point:

> And had your Harvey and our de Graaf seen one hundredth part of what I have seen, they would doubtless have maintained with me, that the seed of the man only forms the fruit, whereas the female's contribution is to receive the male seed and nourish it.

But let not his opponents distort his words:

> I have never said that human sperm is full of little children, but I have said that it is full of live animals or little worms which have long tails and whose shape I have drawn several times; for just as we cannot justifiably call certain worms, whilst they are still swimming in water, flying creatures, although later flying creatures are to come out of these same worms, so we cannot say either, that the little worms which are present in human sperm are small children although out of each little worm a child is to come.

The unanimity of opinion which, since Harvey, had become almost universal—that the whole development of the future animal was centered and performed in the egg—was destroyed. The theory of preformation was still extant, but from now on there would be two schools: the *ovists*—adherents of Harvey, de Graaf, and Swammerdam, who regard the egg as a preformed organism—as opposed to the *animalcultists,* who with Leeuwenhoek saw in the male sperm the future organism in the course of construction. This struggle was to remain undecided for the moment, and gradually it was to decrease in vehemence; but it was to flare up again a century later—at

the time of Caspar Friedrich Wolff, who was a fundamental opponent of every type of preformation and the founder of an entirely new doctrine, that of epigenesis.

During these years, another Dutchman stood side by side with Leeuwenhoek. He was another scientist who has gained a lasting fame through his biological works: Jan Swammerdam. He stood side by side with Leeuwenhoek in a true sense. Although their interests drove them in the same direction, they did not seek contact and seldom mentioned each other's name in their works. In one aspect only were they parallel figures: they were enthusiastic—almost fanatical—investigators of living organisms, devoting themselves wholeheartedly to their scientific task and not willing to be hampered by the commitments of a public appointment. In every other respect, however, they were opponents. Swammerdam was an educated man and a systematic worker who confined his studies to an accurate examination of a few objects only. He had a tendency to mysticism, and was religious—almost superstitious—in addition to being somewhat temperamental. Leeuwenhoek was an amateur, examining with his microscope whatever came to hand or seemed to be of interest. He was objective, realistic, calm and unemotional, and he seldom voiced opinions on religious matters.

In some ways Swammerdam was Harvey's successor, as he continued several of Harvey's uncompleted studies. This he did admirably. His keen insight, helped by the magnifying glass, enabled him to probe into the secrets of the structure of insects and into the successive stages of their development. In the butterfly chrysalis he saw the nearly completed butterfly; he observed that the caterpillar did not contain a structureless, almost liquid mass, but rather organs and tissues just as did the chrysalis and the adult butterfly. By means of his anatomical research, he discovered the true character of the three types of bee: the queen, hitherto believed to be a king, he found to be a fertile female; the workers were sterile females; and the drones were males.

His careful dissection of innumerable insects—e.g., lice, water-fleas, mayflies, dragonflies, ants, bees, wasps, beetles, butterflies, gnats and flies—together with his dissections of spiders, snails, scorpions, worms, frogs, and cuttlefish, gave him a widely varied experience which enabled him to draw up a classification of "insects" based on their development. The fact that he regarded all of these animals as insects whenever he could was a result of his view that

the development of all animals could be considered from a single standpoint. Accordingly, he distinguished: (1) insects which emerge from the egg as adult animals (lice, spiders, snails, worms); (2) insects which come into the world with six legs, but whose wings at first remain hidden under the skin (dragonflies, scorpions, grasshoppers); (3) insects which, after their last moult, pass into a state of quiescence (butterflies, ants, gnats, bees); and (4) insects which, after the last moult, still resemble eggs but leave this larval skin by breaking out as though they were opening a lid (flies). His *Natural History of Insects* (1669), and his collected works, later published by Boerhaave (1737) under the title *Bible of Nature*, rank among the classics of the seventeenth century, not least because of their profusion of detailed and well-finished illustrations.

From insects, Swammerdam leaped ahead to frogs, which he described in a special treatise and which he compared to insects. Between these works came his strange essays, "Comparison of Man even with Insects and Frogs," "Comparison between the Metamorphoses of the Clove Pink, Carnation or Clove-gilly Flower, with the Pupae of Insects," and finally, a tabular "General Comparison and Analogy of the Metamorphoses or Growth of Parts and Limbs, of Eggs, Worms, Pupae or Insects Amongst Themselves; Similarly for One of the Animals with Blood and of Plants in Particular."

In these last chapters, Swammerdam displayed two attitudes which hitherto had remained more or less in the background: i.e., that of the experimental physiologist and that of the philosophical meditator. Frogs furnished him with ample material for experimentation; their musculature and nervous systems gave him opportunities for a number of "very attractive, amusing and useful experiments." The modern animal-physiologist will agree completely with this statement. But the modern biologist shies a little from his pseudo-philosophical discussions, in which he compared the development of flowers, insects, frogs, and man. He concluded from this comparison that it was evident that the animals which have blood —or rather in whose veins red blood flows—were analogous to the metamorphoses of insects, and that in many instances it was also analogous to man himself, for the reason that the works of God appeared to have but one fundamental plan for all reproduction and growth. He found that like the insects, man was born from a clearly-recognizable egg, which after fertilization was transported locally from the egg-nest through the tube to the womb; that this was the

place where man, the reasoning animal, found his first nourishment, like a worm or like a galba in its egg.

There is no need for us to give more of Swammerdam's expositions here. In judging his conclusions, so naive in our eyes, we must bear two things in mind: first, that Swammerdam sought to find in all organisms the common element which in his opinion God had given them and, second, that this common element may be found in the original source of each living creature—which Harvey had found to be the egg. Herein lies the interest of Swammerdam's expositions; here too, is the reason why he, among the adherents of preformation, supported the ovists. This also is partly the reason for his negative attitude towards Leeuwenhoek.

There is unquestionably a certain similarity between six of these seven seventeenth-century contemporaries. Although Redi showed a measure of independence with his experiments on spontaneous generation, he joined Malpighi and Swammerdam in his defense of Harvey's dictum "ex ovo omnia." Leeuwenhoek also accepted the preformation of each organism, but differed from the others in his role as an animalculist. Grew and Malpighi were the founders of plant anatomy. Hooke and Leeuwenhoek were pioneers of microscopy. And Malpighi and Swammerdam prepared the way for detailed anatomical studies.

The last of the seven, however, John Ray (1627–1705), stands entirely apart. He continued the work of the ancient compilers of herbals, especially of those who attempted to create a workable system of classification out of the chaotic mass of names and descriptions of plants—such herbalists for example as Caspar Bauhin. This gave him an understanding of classification. He realized that not only plants but also animals must be brought within a system, and that the fundamental basis of the system must be the same for the classification of both plants and animals. His work was the first to establish the significance of the "generic principle," which forms the basis for every clearly arranged system.

Hitherto, classification had been chiefly built upon two different principles: on practical considerations and on Aristotelian-philosophical principles. Ray tried to combine these two and in this way his work became the starting point for the growth of a sound classification. He began as a botanist. In his first publication, *Table of Plants* (1669), he did not break away from the bondage of a wholly

pragmatic method. Herbs, shrubs, and trees were his main groups and these were further subdivided, the herbs, for example, into *incomplete* (our "Cryptogams") and *complete* (our "Phanerogams"). The complete were again subdivided according to their leaves (long, round, veined, succulent, rough- or smooth-surfaced), their flowers, or their fruits. The shrubs were subdivided into berry-bearing, thorn-bearing, non-thorn-bearing, deciduous or evergreen, siliquose fruits, and grains, and the trees were subdivided into pomiferous, pruniferous, berry-bearing, nut-bearing, coniferous and cupuliferous, corneous, and resiniferous.

But fifteen years later, in his *Methodus Plantarum Nova* (1682), he changed his mind radically. Here his classification, particularly of the herbs, shows a vast improvement. Here the structure of flowers predominates, thus leading to a more modern system. This was even more pronounced in the edition "emendata et aucta" of 1703, where for the first time a differentiation of capital importance was made, that is, between plants with one seed-lobe and plants with two while, contrary to the custom of the writers of herbals, he gave a short diagnosis of the species. His *Methodus Nova* had become more than just a work of classification; he preceded it with a few chapters of a morphological nature, so that the work was more a textbook of botany. This applied to an even greater extent to his great work in three volumes, entitled *Historia Plantarum* (1686–1704).

After the first edition of his *Methodus*, Ray started on a different course. He had piously and meticulously prepared the works of his friend Francis Willughby (1635–1662) for the press. Willughby, dying young, had entrusted to Ray the education of his sons and the editing of his zoological studies—making it possible for Willughby's work to appear posthumously: that on birds in 1676, that on fishes in 1686. At the same time, the editing of the zoological studies extended Ray's interests to the classification of animals. He began his own zoological investigations and, in 1693, published a *Synopsis of Quadrupeds and Snakes* followed by a *Historia Insectorum* (1710) and a *Synopsis of Birds* (1713). His insight broadened and his understanding grew. The generic principle, he saw, must apply equally to plants and animals. He brought the beginning of a new order into a chaos, which his botanical and, especially, his zoological forerunners had been unable to put into shape.

The concept of species, which had still been an uncertain notion in the mind of Albertus Magnus, now acquired a shape and definition:

Amongst animals as amongst plants, there is no other sign of specific affinity than the origin from the seeds of a specific or individually identified plant. Forms which belong to different species, retain their specific character for ever and no one form ever springs from the seed of the other or vice versa.

Ray also realized that the classification of animals should be on an anatomical basis. He distinguished between *vertebrate* and *invertebrate.* The first he subdivided into those with lungs and those with gills. Among animals with lungs he drew a distinction between those with two ventricles in the heart and those with one. The former he divided into viviparous and oviparous. As a result, he obtained this sequence: mammals—birds—frogs, lizards, and snakes —fishes. But he was rather chary of the consequences: he really did not know what to do with whales.

He further realized that the sharpest possible definitions were required, not only for the species but also for the genera and for the terms used. In short he aimed at exactitude in the classification of biology. This made him, more than any other biologist, the chief precursor of and leading contributor to the gigantic task of classification undertaken by the eighteenth-century naturalist, Linnaeus.

Before Linnaeus could carry out his reforms in the classification of plants, however, there was yet another requirement that had to be met: the evaluation of the constituent parts of flowers as reproductive organs—in other words, the recognition of sex in plants. When the botanists of this time spoke of "male" and "female" plants —e.g., *Polystichum filix mas*, which is still called "male fern," and *Athyrium filix femina*, now known as the "lady fern,"—we need not attach any importance to such terms as having a sexual meaning. Such names were given to two similar species by reason of differences in habit, size, or color. A typical example is the difference given by Mattioli between two forms of Dog's Mercury: *Mercurialis mas*, darker in hue than *Mercurialis femina.* But it is to the first that he attributes seed-formation by the flowers, whereas the *M. femina*, according to him, sheds its flowers before they reach seed-formation. He did, therefore, have in mind the two sexes of one of the *Mercurialis* family, but he in fact mistook the one for the other.

Perhaps nothing in the history of science illustrates the difficulty

in establishing an unambiguous priority in discovery so well as the growth of the idea and the final proof that flowering plants reproduce sexually. In 1691, Rudolph Jacob Camerarius (1665–1721) proved the fact experimentally. Long before this, however, a great many botanists had believed in the sexual reproduction of plants; but apparently they did not bother to experiment. Experimentation, as we know, achieved its rightful status very slowly, and for a very long time it was ranked well below plausible philosophical speculations, which sought to discover truth by the use of pure reason.

But reason alone rarely leads anywhere. Reason, by itself, is rarely reasonable. Based on a very little experimentation, reason could have discovered the sexuality of plants some three thousand years before it did.

We have already described how the sexuality of the date palm had been recognized before history started. Moreover, the knowledge of this sexuality was never forgotten. We have cited numerous instances where it was mentioned. Literally hundreds of such records exist. And in addition to these, and often accompanying them, was the vague notion of the sexual significances of such practices as caprification. The question naturally arises, why was the discovery of the sexuality of plants delayed so long? This question is one that is not easy to answer, even though we have no lack of reliable data.

It is not feasible to go into all the ramifications of the problem here. We can mention only a few of the factors that hindered and delayed the discovery. Whenever the ancient or medieval scholars assigned sex to plants the assignation was figurative. Plants could be classified as male or female just as precious stones could be. When two species of a genus were recognized the larger or rougher one was called the "male," the smaller and more refined the "female." Then too, the grafting of one plant upon another supposedly had a sexual aspect, and any chimera or graft hybrid was looked upon as a real hybrid—such as a mule. Moreover, for any ancient or even Renaissance scholar to admit that monoecious plants were both male and female would have forced him to rank the vegetable kingdom as more "perfect" and thus to place it above the animal kingdom. To most philosophers, this would have been unthinkable.

Another factor responsible for the delayed discovery can be designated accidental. In the Old World, the only important dioecious food plant was the date palm, which grew where the European bota-

nists could neither study nor experiment with it. It could not even be grown in Europe, where the spectacular intellectual progress of the Renaissance was taking place.

However, when the European explorers discovered a second important dioecious plant, *Carica papaya*—a fruit tree native to the American tropics—they did record its sexuality and they recorded it accurately. The record, oddly enough came from the Philippine Islands, where the tree had been taken and grown by the Spaniards. Its sexuality was described, however, not by the Spaniards but by the Dutchman, Jan Huygens van Linschoten (1563–1611). The following passage is taken from his account of his voyage to the East Indies. His *Itinerarium ofte Schipvaert naer Oost ofte Portugaels Indien* was published in 1596. (From the English translation, reprinted in 1885, p. 35):

> There is also a fruite that came out of the Spanish Indies, brought from (beyond) ye Phillipinas or Lusons to Malacca, and from thence to India, it is called Papaios, and is very like a Mellon, as bigge as a mans fist, and will not grow, but alwaies two together, that is male and female: the male tree never yeeldeth any fruite, but only the female, and when they are divided, (& set apart) one from the other, then they yeeld no fruite at all.

Other travelers also were impressed by this plant. In 1607, Charles de l'Ecluse was sent a picture and an account of the tree by one of his friends. The Spaniards called the tree *Mamoera* because of the resemblance of the large melon-like fruit to the mammary glands. When the English got to the West Indies and saw the *Mamoera* they distorted the name somewhat, calling it the "mammy" tree. Later, however, the name was changed and, by some strange alchemy, the "mammy" tree became the "papaw."

Caspar Bauhin helped spread the knowledge of this dioecious tree in his *Theatri' Botanici* (1620), and Parkinson (*Theater of Plants*, 1640) followed de l'Ecluse and Bauhin and described the plant under the heading *Mamoera mas et femina*. However, he got the sexes reversed and had the male bear the fruit. The male, he said, was sterile unless the female were nearby. It was John Ray who set the matter straight, describing the male and female trees correctly in his *Historia Plantarum* (Vol. II, p. 1370 [1688]). Thus, in the seventeenth century two important fruit trees were known to be dioecious, and a number of botanists were speculating about the possibility that flowering plants reproduced sexually.

As early as the sixteenth century, however, the sexual reproduc-

tion of plants was proclaimed—but, as far as we know, without experimental support. A Czech physician, Adam Zaluziansky, published *Methodi Herbariae* in 1592. Here he showed that he had a remarkably accurate concept of plant sexuality (pp. 49–50). He stated that all plants and animals reproduced sexually; that in plants, because of their immobility, and also in hermaphrodite animals the two sexes were combined in the same individual.

He stated that in most animals, however, and in some plants, the sexes were separated, and that in the latter case, the dust or pollen from the male flowers had to impregnate the female flower or the plant would bear no fruit. He stated, "The feminine is the plant of the same species [as the male] which produces the fruit, although sometimes with the *Metaphorical significance* explained above, plants of an erect nature with smooth skin are called female." [Italics ours.] Zaluziansky thus recognized that the common classification of plants into males and females was without any real sexual meaning.

It would be needlessly repetitive to cite the many discussions of plant sexuality that were published during the next century. Two of the botanists, however, published works of some historical importance, as it is almost certain that their ideas were known to Camerarius whose outstanding contributions will be described later. The first of these botanists was Nehemiah Grew, already mentioned in this chapter.

In *The Anatomy of Vegetables Begun* (1672), Grew discussed the function of pollen (pp. 137–148) and his discussion helps us to understand the scientific standards of the time. To him the question was, why did God create plants that had pollen? The answer seemed obvious. The larger fruits such as apples, pears, peaches, etc., were created as food for man and for the higher animals in general. The smaller animals—the insects—also needed food, and pollen was created for them to eat. He noted, however, that important as this function was it was only secondary. Pollen, he said had another and primary function that he might discuss later.

In the folio edition of 1682, he described this primary function. The pollen, which his microscope had shown him to be composed of small, perfect globes, varying in size and markings from species to species, was the male element in flowers. These grains of pollen fell upon the "uterus" of the flowers and fertilized them in a manner analogous to the coition of animals.

Grew's idea was accepted by John Ray, who quoted Grew in detail and who described sex in plants many times. He recognized that most plants were like hermaphrodite animals, but that, in some species, the sexes were separated. He noted, moreover, that some plants bore two types of flowers, each type containing either male or female elements but not both. In such plants only the female flowers developed into fruit. (*Historia Plantarum*, Vol. I, pp. 16, 17 [1686]; Vol. II, pp. 1353, 1354 [1688]). Camerarius, as we have stated, almost certainly knew of the ideas of Grew and Ray. His contribution was to supply the experimental proof.

The theory that prevailed before the work of Grew and Ray was published was that the calyx, corolla, and stamens were the organs which served to protect the bud that was formed inside and to remove surplus and injurious substances. This concept could only be refuted by experiment. December 28, 1691 is the date given by Camerarius to the first of a series of statements which he published about his experiments on pollination and fertilization in plants. He had observed that while a female mulberry tree would yield fruit even if there were no male tree in the vicinity, all the seeds in the fruits were sterile. In other words, they did not contain germs. Consequently, the first question he asked himself was this: can dioecious plants, when absolutely isolated, produce mature seeds? To answer this, he potted two seed-bearing plants of Mercurialis and placed them indoors. The growth and flowering of the plants continued undiminished; the fruits formed, and subsequently began to swell, but soon dried up so that not a single ripe seed was produced. Naturally, he came to the conclusion that female plants must not be isolated from the males if they are to yield mature seeds.

Important for the spate of material it contains is his *De Sexu Plantarum Epistola* (1694), in which he recorded a large number of observations not only on dioecious but also on monoecious plants; e.g., the castor bean and Indian corn (maize). Here he saw that seed-formation was absolutely out of the question if the male inflorescence were removed before it opened or, in the case of maize, if the long styles of the female inflorescence—the corn silk—were cut off.

His proofs were absolutely conclusive for his inferences "that in the realm of plants no reproduction by means of seeds—the most perfect gift bestowed by nature, the universal means of maintaining species extant—can take place if the stamen has not previously gen-

erated life in the germ present in the seed." It is typical of his accuracy that, in spite of his convictions that his thesis was correct, he felt compelled to mentioned the exceptions: isolated hemp plants with which he experimented did yield seed; one of the maize plants yielded a few grains, although the styles had been cut off. For this he could find no solution, but—and rightly—it did not cause him to abandon his hypothesis. The work of Camerarius has assumed classic importance. Others were to continue to build on it and probe more deeply into the question of the sexuality of plants, but he remains the founder of a valid theory.

Thus it is the work of this circle of seven, with one standing apart, which turned the seventeenth century into the period in which a true science of biology came into being. This period no longer had any use for mere hypothetical, philosophical speculations. Observation and experiment demanded their rightful places. At first this attitude was somewhat limited and adverse to anything resembling philosophical reflection, yet gradually it became evident that observation and experimentation alone did not suffice for the development of science. It was virtually inevitable that this young and vigorous development should lead to individual conceit. It is this self-assurance which one senses in Baglivus' pronouncement (1701): "It would be disgraceful in a century as enlightened as ours, where experience and the firmly-established teaching of mathematicians have thrown so much light on the origin of things, it would be disgraceful, I say, for a philosopher or a physician still to believe in Galen."

9

Depression and Revival

The climax of seventeenth-century biology came and passed. In its later decades few important discoveries were made, and this decline was to continue for a number of years. The first quarter of the next century would also be somewhat sterile, because interest in biology was still on the wane. For the next fifty years, few works of any importance left the press. But improvement came slowly, step by step, and eventually the years of moral inertia passed; biology lived again.

At first, progress was intermittent: the work of Swammerdam and Leeuwenhoek had led to a closer observation of natural phenomena, and this trend was continued by the early eighteenth-century zoologists. Studies of insects by A. J. Roesel von Rosenhof (1705–1759) were published in his *Monatlich herausgegebene Insektenbelustigung* (4 vols., 1746–1761), and his work on the development of the frog appeared in *Historia Naturalis Ranarum* (1758). Then there was Pieter Lyonet (1707–1789) of Maastricht, whose *Traité Anatomique de la Chenille qui Ronge le Bois des Saules* (1750) was a masterly anatomical study of the goat-moth caterpillar. It is a remarkable fact that neither Roesel nor Lyonet was really a biologist; instead they were draftsmen and engravers who, attracted by the delicate structure of insects, were lured into natural research. Leeuwenhoek's line of research was continued by M. F. Ledermüller (1719–1769), who, in his *Mikroskopische Gemuts und Augenergötzungen* (1761–1763), has left us some excellent illustrations of the organisms he called "infusoria."

Whereas this work was a zoological study of the lower animals only, and merely followed the lines laid down by earlier scientists, research into the higher animals, however, developed along other lines and began to separate into physiological and morphological-anatomical fields of specialization. The leading scientist in the physiological field was a Swiss, Albrecht von Haller (1708–1777), a man of many talents—poet, philologist, physician, zoologist, and botanist. He was ranked among the foremost naturalists of his time. Although he was autocratic, he could be amiable on occasions. More often, however, he lacked an appreciation for the other eminent men of his time. The story goes that Voltaire congratulated a visitor, who had met Haller, on the honour of having met so great a man. When the visitor showed considerable surprise, since Haller had expressed a contrary opinion of Voltaire, the latter said: "Oh well, then we must both have been mistaken."

Haller was especially interested in physiology where, in addition to his excellent work on the sensitivity of the nervous system, he has to his credit a revolutionary textbook entitled *Elementa Physiologiae Corporis Humani* (1758). This work justly stamps him as one of the founders of modern physiology and one of the protagonists of the view—then somewhat novel—that the physiology of animals has a claim to our attention equal to that of their anatomy. As a physiologist, he was more or less the spiritual successor to Harvey. He was also interested in botanical research and wrote a number of impressive works, particularly on the flora of Switzerland and of his hometown, Göttingen. These works bear witness to his wide knowledge of plants.

Throughout this century, morphological work on a classical scale was truly international. The Dutchman Petrus Camper (1722–1789), the Scotsman John Hunter (1728–1793), the Frenchman F. Vicq d'Azyr (1748–1794), and the German J. F. Blumenbach (1752–1840) comprise a quartet who were to follow in the steps of Koyter and the other seventeenth-century anatomists, and who were to establish the comparative anatomy of the vertebrates as a science in its own right.

The elegant yet libertine figure, Petrus Camper, was the most original of the four. Financially independent, he accepted professional appointments only to lay them down again whenever the duties of the post were not to his liking. He was a florid writer, an eloquent speaker, and a masterly draftsman, and he presented his

apparently dry anatomical data in such a way that it is still fascinating, even to the modern layman.

He examined, for example, the organ of hearing in the "scaled fishes" (1762) and compared it to the organ in a sperm whale and to the organ of land animals. He studied the hollow bones of birds, comparing high-flying birds of prey with diving birds such as the penguin and with such cursorial birds as the ostrich and the cassowary. He analyzed the skeleton of an orangutan and used this research as the starting-point for a comparison between all the available primates from apes and prosimians to man. He drew particular attention to the significance of the facial angle. At Groningen University he delivered an inaugural lecture on the analogies between animals and plants (1764) in which he discussed in poetic language the similarity between their vascular systems, glands, and reproductive organs, pointing out the occurrence of sensitivity in plants and the possibility of their possessing a diffuse nervous system.

Camper's work emphasized the similarity and difference of organs within closely related animal groups. Hunter and Vicq d'Azyr, more or less under Haller's influence, advocated this type of comparative research, provided that an analogy in the functioning of the organs could be assumed. Their starting-point was the idea of a common factor in the structure of all animals. This led them to compare analogous organs in different animals and thence different organs in the same animal.

The contribution of the fourth member of the quartet, Blumenbach, lay chiefly in his profound studies of man in all his many varieties. He thus became the founder of anthropology.

Blumenbach, however, did not establish the science of anthropology *de novo*. He had predecessors, even in antiquity. The classical philosophers had described the different races of men who lived around the Mediterranean Sea, and they described them accurately. But they also wrote about the queer types and monstrous races who lived just beyond the farthest horizons. Pliny, especially, has left us some vivid accounts of these mythological men who, however, tended to evaporate when the later European explorers visited and observed the far lands where they were supposed to live.

But the explorers did discover real races in the Americas, in the Far East, and in Africa south of the Sahara. They also discovered the anthropoid apes. Before long, the striking resemblances and trivial differences between the apes and man could not be ignored

by the scientists who were laying the groundwork of anthropology.

When the Spaniards had finally convinced themselves that they had discovered a "new world," they naturally wondered where the inhabitants had come from. Were they descended from Adam, or were they in some unexplained way preadamites? After much debate they decided that all true men were descended from Adam. Then the question arose: how had the various races come to differ so much from the Europeans and from each other? Almost without exception the question was answered by assuming that the various races had inherited the adaptive changes that had been forced on their ancestors by the many odd climates in which they had lived.

We need not list here the numerous records of this explanation of our human racial differences. It was accepted routinely and universally. But the discovery of our sub-human relatives, the anthropoid apes, raised questions that could not be answered so easily. Even in antiquity the ape could not be dismissed lightly, and in the seventeenth and eighteenth centuries, when all five genera of the Homidiae were encountered, the ape demanded attention. Was he or was he not a man? Obviously he should be studied thoroughly.

Late in the seventeenth century a truly excellent work on the chimpanzee was published by Edward Tyson (1650–1708). It was entitled *Orang-Outang sive Homo Sylvestris,* or the *Anatomy of a Pygmie Compared with that of a Monkey, an Ape and a Man* (1699). This book was the first detailed and accurate account of a manlike ape. It was a major contribution to physical anthropology. Tyson deserves all honor.

Tyson also made some valuable contributions to comparative anatomy. He published papers on the anatomies of the porpoise, the rattlesnake, the opossum, and the Mexican muskhog. He dissected and described the annelids, or segmented worms, and published important studies on those round worms that were parasites. He was an exceptionally able and versatile man, but we remember him mainly because of his anthropological contributions.

Thus, as an anthropologist, Blumenbach had much to build upon, and he built well. His *De Generis Humani Varietate* (1775) contained the best classification of the human races up to that time, although during the last thirty years of the century, works on the human races were numerous. Much earlier, Linnaeus (1735) had classified mankind and the apes, putting them together in the order *Primates.* Even before this, there had been some rather casual list-

ings of the races, such as that of Vincentius Rumpf (*De Hominibus Orbis Nostri Incolis, Specie et Ortu . . .* 1721). Richard Bradley in *A Philosophical Account of the Works of Nature* (1721), a rambling collection of essays, printed a fairly accurate illustration of a human and a monkey skeleton shown side by side (Fig. 17, p. 142).

Thus comparative anatomy, whose beginnings we owe to Koyter, was developing into a distinct and separate discipline, thanks to the contributions of such men as Tyson, Camper, Hunter, d'Azyr, and Blumenbach. The whole outline structure of the science would be completed in the next century by the genius of Cuvier.

Other fields of zoological research were also being prepared for cultivation. The famous and versatile French physicist, R. A. F. de Réaumur (1683–1757), the man who invented the alcohol-thermometer, was one of the first to extend and develop the work that had been begun by Swammerdam. Réaumur, however, approached the subject from an entirely different angle. He was a wealthy amateur who, literally, interested himself in all science. He improved iron refining and contributed to our knowledge of specific heat. He investigated the factors responsible for the expansion of gases. He studied the mollusks and the formation of pearls. By assuming a kind of pangenesis, he explained how crustaceans could regenerate an appendage that had been lost. (Some gemmules of the lost portion had remained in the rest of the body.) He also described the artificial incubation of eggs.

In his six-volume *Mémoires pour Servir à l'Histoire des Insectes* (1734–1742), he recorded not only the accurate results of anatomical research but also opened up a hitherto undiscovered path of biological study. He verified the work of Swammerdam, who had differentiated between the three types of bees (queen, workers, and drones). He did this by inventing a hive with glass walls so that he could observe the secrets of the bee community. Thus he was able to affirm that what had formerly been thought of as a "king" bee was actually a "queen," and was, in fact, the only fertile female in the hive. He recognized the workers as sterile females and he identified those formerly described as "fuci" as the males. He made the important discovery that plentiful nourishment can make any female larva develop into a queen. His work illustrated successfully the fact that the anatomical data from insects could be supplemented and added to by the study of their habits. His work was the first attempt at research into animal sociology.

At this time, a second means of extending the field of zoology was discovered by the Swiss, Abraham Trembley, who, while he was tutor to a family in the Hague, wrote his *Mémoires pour Servir à l'Histoire d'un Genre de Polypes d'Eau Douce à Bras en Terme de Cornes* in the years 1740 to 1743, published in 1744. Ever since ancient times there had been organisms which, because of their indefinable characteristics, were obstacles to the systematically inclined zoologists. Were these organisms to be counted as plants or as animals? What were corals and polyps, and what was that freshwater polyp *Hydra* (Figure 40), a sessile organism, sometimes green in

Figure 40. Fresh water polyps. (From Trembley, 1744.)

color, but in other ways behaving exactly like an animal? The body of these freshwater polyps may be compared to a hollow cylindrical tube, closed at the base and bearing eight tentacles round the opening at the top. Apart from this they had the added peculiarity that they were able to multiply with the utmost ease by forming buds, each of which could develop into a new and complete organism.

This method of reproduction gave Trembley the idea of investigating the problem as to how many pieces one hydra could be divided into, and to what extent these pieces would be capable of

re-organization—that is, of regenerating a complete animal. He began his first experiment by cutting the polyp transversely in halves. The top half, which bore the tentacles, immediately closed up at the base and suffered a very slight interruption of its life processes. The lower half, now open at the top but bereft of tentacles, gradually formed new tentacles, at first three or four, then all eight. After two days it was able to feed with their help. In this way, by artificial means one organism could be made to become two, and all the missing parts could be replaced by regeneration. Cutting the polyp into three or four cross-sections had the same result; three or four small but complete polyps were formed.

If the section were not made horizontally but vertically, thus splitting the tube into two open halves, each part would have a few tentacles. Once more regeneration began; the tubes closed up and the arms that were lacking grew until there were eight once more. By cutting vertically, but stopping short of the closed base of the organism, Trembley even managed to cause the formation of branched multi-headed polyps. Finally, by inserting a hog's bristle into the cavity of a polyp, he managed to turn the whole animal inside out, like the finger of a glove. At first the polyp did its best to recover its previous form. When this was prevented, it lived on happily with its new arrangement. It began to eat again; it grew and sprouted buds. By performing these exceedingly clever and original experiments, Trembley was the first to enter the realm of experimental morphology, a branch of biology which attempts to change the exterior shape of plants or animals by experimental intervention and to trace the connections between the altered circumstances of the organisms and their behavior.

In this new field of experimentation, Trembley immediately aroused the interest of Réaumur, who fully understood the significance of his work. Another important successor to Trembley was Charles Bonnet (1720–1793) who, in 1745, published his *Traité d'Insectologie, 2ᵉ Partie: Observations sur quelques Espèces de Vers d'Eau Douce*, describing his experiments on regeneration both in fresh-water worms and in earthworms. He was convinced that transverse sections of these animals could regenerate just as the polyps did; the posterior cut edge into an anterior cut edge.

Twenty years later, in 1768, Lazarro Spallanzani (1729–1799) (*Prodromo di un Opera sopra le Riproduzioni Animali*) related his experiments on the regeneration of other invertebrates, e.g., on the

edible snail, and on the regeneration of the vertebrates, e.g., on the newts. Here he observed that a single animal could replace its legs or its tail, whichever had been cut off, as many as six times, but that the process was much easier and quicker in young animals than in older ones.

The correspondence and intellectual contacts between Réaumur, Bonnet, and Spallanzani again created a center of scientific activity in which there developed a lively interest in all kinds of problems, particularly the physiology of reproduction. These investigators told each other of their experiences and exchanged their views about what they had discovered. They soon started to investigate two subjects of fundamental importance, testing their theories about these subjects in the light of what each had done. The objects of their interest were two age-old problems: (1) *spontaneous generation*, and (2) the preformation of the organism before it begins to develop.

Long ago the Greeks had speculated about the generation of living organisms out of inanimate substances—i.e., *generatio spontanea*. As far as the higher animals were concerned, the problem had been solved by Harvey with his magic phrase "ex ovo omnia" and subsequently by Redi with his experiments on flies. At least, it had been solved to the extent of demonstrating that the flowering plants and all the animals, from vertebrates to insects, originate in an "egg." Thus they could not generate spontaneously from inanimate constituents. But Leeuwenhoek's work had pushed the frontier of this problem further back, in that he had discovered the realm of the invisible. He had found the little, microscopic "animalcules" which we now know as bacteria and infusoria. And the problem of spontaneous generation must also include the origin of these. Was it possible for organisms of such small dimensions to spring spontaneously from inanimate substances? Leeuwenhoek had said that this would be a miracle. But there existed such a possibility. This possibility was very attractive to those who were anxious to accept a "miracle" if it would save the views they held so dear. It may have been true that, since Redi's experiments, spontaneous generation no longer applied to insects, but it seemed reasonable for bacteria and infusoria to originate from inanimate substances.

In 1745, the English clergyman John Turberville Needham (1713–1784) published *An Account of Some New Microscopical Discoveries,* based upon the results of his experiments. He had

made infusions of all kinds of substances, animal as well as vegetable, which, in spite of being submitted to intense heat for half an hour in sealed tubes, were seen to contain a multitude of microscopic organisms. Needham assumed—justifiably, it would seem—that the germs of all living organisms must be dead after such a treatment. What he found, therefore, he could explain only by a process of spontaneous generation. Needham's theories were accepted by the famous Buffon (1707–1788), who was a writer rather than a scientist, a journalistic propagandist rather than an original investigator. Through Buffon's influence, and also through his own popularity in French "salon" circles, Needham's statements were soon invested with the character of a scientific dogma.

Only a few had sharp enough critical faculties to arm themselves against an immediate acceptance of Needham's views—Bonnet, on the more philosophical side, and Spallanzani, whose excellent experimental work on regeneration we have mentioned. Both of these scientists turned their attention to the problem of spontaneous generation. Spallanzani repeated Needham's experiments more accurately and communicated the results in his *Saggio di Osservazioni Microscopiche Concernenti il Sistema delle Generazione de Signori di Needham e Buffon* (Modena, 1766). These results were the exact reverse of the generally accepted dogma.

According to Spallanzani's account, Needham had not been sufficiently accurate in his experimentation; instead of sealing his glass tubes with cork and sealing-wax, as Needham had, Spallanzani placed his infusions in glass tubes which he sealed by melting the openings. After this treatment no infusoria or bacteria appeared. Needham had heated his infusion for half an hour, Spallanzani for three-quarters of an hour, and this increase in the time was the cause of the extermination of all the germs. Needham's half-hour had simply not been long enough. Spallanzani's proofs should have been entirely convincing to objective judges. To the general masses, however, influenced especially by Buffon's eloquence, Needham's apparent "proof" was valued above Spallanzani's negative but conclusive results. The battle continued; the decisive solution was to come a whole century later with Pasteur.

On the face of it, the problem of spontaneous generation seems to have little connection with that of preformation. Yet this view is deceptive. Fundamentally, both problems are built around the question as to whether an organism exists always as a germ cell. It

is for this reason that Bonnet, the most philosophical of this eighteenth-century group, considered both problems together. As a result of his logical application of Harvey's "ex ovo omnia," he postulated both the impossibility of spontaneous generation and the truth of that particular preformation theory that was held by the ovists.

Everywhere Bonnet found support for this theory, which he elaborated much further than the seventeenth-century ovists had ever done. He published his conclusions in his *Considérations sur les Corps Organisés* (1762). In this he traced the ovist theory back to two hypotheses: first, that of "emboîtement," which assumes that the germ cells of all individuals of a species lie encased in another germ cell—in that of its mother—and develop or "evolve" gradually. In all these germ cells lie more germ cells, enough for all the following generations, and so on. *Ad infinitum?* He was aware of the great difficulty inherent in this notion and therefore limited each species to a restricted span of life, although it might be several thousand years long. His second hypothesis was that of "dissémination," based on the assumption that there are germ cells everywhere and that no living organism is ever generated from non-living matter. The concept of spontaneous generation he thus found to be inadmissible. He did not know what to make of Leeuwenhoek's "spermatist" theory so he practically ignored it. But the "ovist" doctrine of Harvey, Redi, Malpighi, and Swammerdam he accepted with alacrity and applied it to his zoological data.

On the botanical side Bonnet got the support of Michael Adanson (1727–1806), who, in his *Familles des Plantes* (1763), objected to Camerarius' conclusions that the pollen penetrates the style of the flower on the ground that there are numerous plants with solid styles in which there is no space available for the pollen grains to enter. This in itself would go to prove that it was not the intromission of pollen dust which effects the fertilization, had it not already been proved by microscopic observation that the embryo was ready-formed in the seeds of plants, which have not been fertilized. Bonnet with some satisfaction then concluded that actual observations had indicated that in vegetables as well as in animals, the germ cell habitually belonged to the female.

All the same, the days of this ovist doctrine were numbered. It was incompatible with the results of some genetical observations recorded by P. L. M. de Maupertuis (1698–1759) and also with some

of the advances in animal embryology made by C. F. Wolff. Finally it was shown to be incompatible with the results obtained from the plant hybridization experiments of J. G. Koelreuter.

Meanwhile the preformationists saw in all germ cells—the ovists in the eggs, and the animalculists or spermatists in the spermatozoa—the whole structure of the organism, but in miniature. The sole factor required for the physical development of these fully-formed organs they assumed to be an increase in size; the only exterior factor needed was nourishment. Simple, clear, and reasonable as this concept was, it turned out to be false. It could not accommodate the discoveries of Maupertuis.

In all of his work, Maupertuis showed that he had an exceptionally clear understanding of the scientific problems of his time, but his very real contributions to biology have been overlooked until quite recently. Many historians of biology have ignored him completely, although he was a well-known and even famous scientist in his day. He was the Head of the Berlin Academy of Science. He had the misfortune, however, of arousing the wrath of Voltaire, who satirized him mercilessly. From then on, he was simply ignored.

Philosophically, Maupertuis was a mechanist. He discarded all vitalistic explanations of vital phenomena, including the teleological explanations of biological adaptations. In place of teleology, he visualized the workings of at least the negative aspects of natural selection. Again, and following the precedent set by Empedocles, Lucretius, and his own contemporary, Denis Diderot (*Lettre sur les Aveugles,* 1749), he explained the existence of adaptation by showing how all unadapted creatures would have to become extinct. Diderot had written (*Oeuvres,* Paris, 1818, Vol. I, p. 319):

> If the first man had had a closed larynx, had lacked suitable food, had failed through the parts of generation, had not met his mate, or had spread to another species, what would have become of humanity? It would have been enveloped in the general purification of the universe; and this proud being who calls himself man, dissolved and dispersed between the molecules of matter, would have remained perhaps forever, in the ranks of possibilities.
>
> If there had never been malformed creatures, you would not fail to insist that there will never be any; and that I am throwing myself into a chimerical hypothesis: but order is not so perfect, that there do not still appear, from time to time, monstrous productions.

This negative and destructive aspect of natural selection applied even to teleologists themselves, and consequently no teleologist could logically use his own adaptation as evidence of the existence

of some universal and well-designed cosmic plan, because the teleologist himself had to be adapted personally if he were to remain alive. This meant that he was not a mere chance sample of things in general. He could not assume that he himself was typical. Obviously no group of unadapted philosophers could ever meet together and argue learnedly as to why it was that they were extinct.

Maupertuis developed the idea further (*Essaie de Cosmologie*, 1750, p. 11):

> . . . . May we not say that, in the fortuitous combination of the productions of Nature, since only those creatures *could* survive in whose organization a certain degree of adaptation was present, there is nothing extraordinary in the fact that such adaptation is actually found in all those species which now exist? Chance, one might say, turned out a vast number of individuals; a small proportion of these were organized in such a manner that the animals' organs could satisfy their needs. A much greater number showed neither adaptation nor order; these last have all perished. . . . Thus the species which we see today are but a small part of all those that a blind destiny produced.

In his *Venus Physique*, published anonymously in 1745, he had gone a step further. He explained the origin of the various human races as due to the preservation of those chance variations which marked off the pigmented peoples from the original white stock from which they supposedly had sprung. He suggested also the possibility that acquired characters were inherited and that, whenever they were valuable and adaptive, they would aid in creating the newer divergent types. But to Maupertuis this kind of inheritance was only a subordinate factor in the formation of the new races.

Maupertuis had noticed that many inherited traits could pass separately from one generation to another, and he concluded logically (and correctly) that the machinery of heredity had to be particulate. Some modern biologists (Glass, 1947) have even seen in these hereditary particles an adumbration of Mendelian genes. Maupertuis, however, made the nature of his particles crystal clear. They were identical with the particles that Buffon has postulated in his conception of pangenesis. Indeed, it is a question as to whether Maupertuis got his idea of pangenesis from Buffon or whether Buffon got his from Maupertuis. In his *Systeme de la Nature* (1751) Maupertuis stated, in Section 33:

> The elements adapted to form the foetus float in the seeds of the father and mother animals: each extract of the part, similar to that which it is to form, maintains a kind of souvenir of its original condition; and it will tend to take it back again wherever it is able to form the same part in the foetus.

Mendelian genes do not behave this way.

Maupertuis thus endorsed pangenesis and in doing so denied the preformation of the embryo. He was, in fact, a leading epigenecist. Already, in his *Venus Physique,* he had recorded many instances in which the offspring had inherited qualities from the father and also from the mother, and in his rather extensive but unsystematic studies of hybrids he had shown that neither the ovists nor the spermatists could possibly be right. The hybrids he studied were, as a rule, intermediate between the parental races, hence they could not have been pre-formed in either.

The fact that hybrids resembled both their parents was incompatible, of course, with preformationism, but Maupertuis was not the first to point this out. Patrick Blair preceded him by a quarter of a century when he published *Botanick Essays* (1720). The next year, Blair (*Phi. Trans.* 31:215–224) called attention to the fact that different combinations of unit characters occurred in plant hybrids and he stated that this fact could not be explained by the preformationists.

The strongest argument that Maupertuis found for epigenesis lay in a pedigree he published of a German family, many of whose members were polydactylous, and in the *Systeme de la Nature* (1751), he recorded the fact that six-fingered hands could be inherited either through the father or through the mother. In the same year, Réaumur (*Art de Faire Éclorre . . . des Oiseau Domestiques,* edition of 1751) also described the pedigree of a family in which an extra digit was transmitted through either the male or female line. But neither Maupertuis nor Réaumur was the first to publish a pedigree of polydactyly.

At this point we have an almost unprecedented opportunity of pointing out a bit of historical irony that did *not* occur. The case in point was a disturbing fact that could have confused both Réaumur and Maupertuis: it was a pedigree of polydactyly that had been published a century earlier—but fortunately, neither Maupertuis nor Réaumur knew of it. Sir Kenelm Digby published the *Immortality of Reasonable Souls* in 1645. Here (p. 136) he described a pedigree of a family in which an extra thumb on the left hand was inherited by all of the women (eight) but by none of the men. His pedigree showed that for five generations, the extra digit was inherited exclusively through the female line and that all the females

showed it. In Drosophila this type of inheritance is easy to explain. We only have to assume that a pair of X-chromosomes were joined together and that the pair carried the gene in question. In man, however, the attached X's would be accompanied by a Y-chromosome, and the individual would not be female, as in Drosophila, but a sterile male. On a straight Mendelian basis this inheritance, however, would have been "highly improbable." We can imagine, though, how this pedigree would have delighted the ovists. Fortunately, however, by remaining unknown, it did not complicate the refutation of preformationism.

Caspar Friedrich Wolff (1733–1794) in his *Theoria Generationis* (1759) also expressed his wholehearted disagreement with the conception of preformation. His embryological research on animals and on the meristems of plants led him to the conclusion that there was no structure of any kind in the germ cell. At the outset, he held that the plant and animal substance consisted of a jelly-like mass, in which small cavities were gradually formed, cavities whose walls had a tendency to become rigid. This rigidity, he found, was much more noticeable in plant than in animal tissue. The consolidation of this structureless material brought into existence the "cells" that had been discovered by Hooke and seen so clearly in plants by Grew and Malpighi.

Although this hypothesis concerning the formation of cells has proved to be completely erroneous, it undoubtedly had some value in a preliminary attempt to investigate the cell as the basic unit of all organisms. The question was pursued further: what caused cavities to appear in this jelly? To explain this, Wolff presupposes a *vis essentialis corporis*—an essential corporeal force "which causes fluids to be collected from the surrounding soil to be sucked up through the roots, spread through the entire plant or accumulated in certain parts of it or separated and excreted." The stream of these fluids, he said, penetrated the mass of jelly. Flowing slowly, it disintegrated into droplets, which formed the bubbles and made the surrounding walls grow rigid. A swifter flow caused protracted tubes instead of bubbles. These were the vessels in plant tissue.

This *vis essentialis* was something mystical. According to Wolff, it was capable, together with the principle of consolidation, of determining the whole structure of the plant or animal. This mysterious activity of the "essential force" therefore created out of the

entirely homogeneous, structureless raw material of the germ a complex of spaces which were grouped together and synthesized as tissues and organs, thus building up a structure.

This is the essence of the theory of *epigenesis,* which Wolff defended against the theory of preformation. He was only 26 years old when he expounded this theory; and, indeed, it bears traces of being a youthful work. He was a philosophical adherent of the school of Leibnitz, who was ever a vehement antagonist of preformation. Wolff was a competent observer but he was apt to underestimate the difficulties of the material he was investigating. Although he had a violent disposition, he was full of respect for the opposing theories of others—especially of the men of some authority—provided, of course, that their theories were well founded. This is borne out by a statement he made in a letter to his opponent Haller: "We only investigate for the sake of truth; we are both seeking the truth, why then should I fight you?"

For Haller, even more than Bonnet, was a formidable opponent of Wolff. Perhaps this should surprise us, as Haller was by no means an out-and-out morphologist. His philosophical tendency, like Wolff's, was vitalist. But Haller was so convinced of the value of morphology that he would always take up the cudgels in the struggle between preformation and epigenesis, and always in favor of the former doctrine.

Other obstacles were laid in the path of the preformationists besides the new theories of Wolff. The chief of these, as we have stated, came from the existence of hybrids intermediate between the parental stocks. Animal hybrids, of course, had been known from the earliest times. Ancient Greek writers such as Empedocles and Aristotle, Romans such as Varro, Pliny, and Galen, and writers of the Middle Ages such as Albertus Magnus and Gesner described not only such hybrids as mules and hinnies but also a number of fantastic cross-bred monsters, which existed supposedly in North Africa. They described centaurs and chimeras, as well as hybrids between an eel and an adder, between lions and leopards, and between bulls and mares. The giraffe was described as the hybrid between a camel and a panther, the hyaena as that between a dog and a wolf, and the manatee as a cross between an Arab and a fish.

Similarly, with human beings, women supposedly could bear the offspring of apes—especially baboons—and of bears, dogs, and goats. Human races could, in reality, interbreed. In some manuscripts we

find information mentioning accurate details of characteristics derived from the fathers: the color of hair in offspring of white women and Ethiopians, the color of fur found on the offspring of tame rabbits which have been mated with the wild, normally-colored males. Thus Ray wrote in 1691:

> But to what shall we attribute the Foetus its likeness to the Parents, or omitting them, to the precedent Progenitors; as I have observed some Parents that have been both black hair'd, to have generated most red hair'd children, because their Ancestors hath been of that Colour? Or why are Twins so often extremely alike? Where this is owing to the efficient, or to the Matter?

Of especial importance to the history of genetics is a statement of Leeuwenhoek, made in his 38th letter to the Royal Society (July 16, 1683):

> Many of our citizens keep rabbits, some for their amusement, others for profit; and these are usually large white rabbits with very long ears, which they consider one of the beauties. To make white rabbits produce grey ones, which they are wont to sell in spring as wild rabbits, they mate a grey male with the white females, which grey male, a young one, had been caught by them in our sand-dunes (where all the rabbits are grey) and they mate this grey male, which they call a "permanent grey" not only with the white but also with mottled, blue-grey and black females and all the progeny resulting therefrom is of the paternal grey hue. Nor has it ever been seen that any of these young had a single white hair or hairs any other color but grey: and moreover they never grow as large as their mothers nor do they have ears as long as hers, nor will they ever be quite as tame as the Mother but always something of wildness in their nature.

It was therefore certain that the paternal animal influenced the appearance of the offspring and this could not be reconciled with a strict ovist theory. But the offspring also showed characteristics of the mother, and this was contrary to the spermatist theory. An epigenesis theory such as Harvey's could explain this no better, although, because of its comparative vagueness, his explanation met with fewer obstacles.

Apart from these few exceptional statements made by Ray and Leeuwenhoek, the zoological literature of the period was extremely vague on the subject of hybrids. On the other hand, there were statements from botanical sources at the time which showed that neither the egg alone, nor the male germ cell alone, could form the offspring. In 1696 Camerarius had proved experimentally the existence of sexuality in plants. In 1761 Joseph Gottlieb Koelreuter (1733–1806; Figure 41) published his: *Vorläufige Nachricht von einigen das Geschlecht der Pflanzen betreffenden Versuchen und*

Figure 41. Joseph Gottlieb Koelreuter (1737–1806).

Beobachtungen, to be followed by three sequels (1761–1766; Figure 42). Koelreuter's researches were to throw an additional and very different light on the question of sexuality in plants. He advanced our knowledge of the subject far beyond anything that Camerarius had done. His work has become the basis of all modern knowledge of plant hybridization. He investigated the behavior of the progeny of individuals belonging to two or more kinds or varieties, and in some instances between two or more species.

Koelreuter's writings show him to have been a man of great scientific versatility. Besides the question of hybridization—the

Dritte Fortsetzung

der

vorläufigen Nachricht

von einigen

das Geschlecht der Pflanzen

betreffenden Versuchen

und Beobachtungen,

von

Joseph Gottlieb Kölreuter

der Arzneywissenschaft Doctor, Hochfürstl. Baden-Durlachischen Rath und Professor der Naturhistorie.

Leipzig,
in der Gleditschischen Handlung,
1766.

Figure 42. Title page of Koelreuter's work on plant hybrids (1761).

primary purpose of his research—he recorded a number of observations about the relationship between insects and flowers, about wind pollination, about the importance of nectar in pollination, and about the phenomenon of *protandry*—whereby the stamens ripen before the stigma, so that a flower could only be fertilized by pollen from a younger flower.

But Koelreuter's talent reached its real height only in purely experimental work: the process of artificial hybridization. Here he showed himself a master, displaying a skill entirely lacking among both his predecessors and his contemporaries. The innumerable hybrids he produced and described give a vivid picture of his capacity for work, the accuracy of his observation, and his methodical approach to the questions requiring solution. He recorded with some astonishment the greatly increased vigor of a number of his hybrids. Hence we can credit him with discovering hybrid vigor even though he did not exploit his discovery in any way.

He did not confine himself to the mere production of hybrids; he investigated also the behavior of hybrids in subsequent generations. His experiments led him to conclude that, as far as plants were concerned, neither the ovists nor the spermatists nor yet the believers in epigenesis could be right. Again, nearly a whole century was to pass before Koelreuter's negative answer was to be succeeded by a more positive conclusion.

Koelreuter had occupied himself chiefly with the experimental investigation of hybridization. His observations on the process of pollination were to him of only secondary importance; yet these observations served as a major stimulus to the study of the part played by insects in the fertilization of flowers. Several historians of biology have even credited Koelreuter with having discovered insect pollination. But insect pollination was well known long before he studied it. Forty years earlier it had been described precisely and accurately by Philip Miller (1694–1771).

Philip Miller was the head gardener of the Chelsea Physic Garden, and the author of the famous *Gardener's Dictionary* (1731 to 1804, nine complete folio editions, five abridged quarto editions, and editions in Dutch, German, and French). It was first published in 1724 under the title of *The Gardener's and Florist's Dictionary.* Miller's first description of insect pollination was in a letter he wrote to Richard Bradley dated October 6, 1721. This letter was published by Bradley in *A General Treatise of Husbandry and Garden-*

ing (1726, Vol. I, p. 332), although Miller had already published it himself in the 1724 edition of his *Dictionary*. This excerpt from Miller's letter is worth quoting:

> About two days after, as I was sitting in my Garden, I perceived, in a Bed of Tulips near me, some Bees very busy in the Middle of the Flowers; and viewing them, I saw them come out with the Legs and Belly loaded with Dust, and one of them flew into a Tulip that I had castrated: Upon which I took my microscope, and examining the Tulip he flew into, found he had left Dust enough to impregnate the Tulip; which when I told my Friends, they concluded that theirs might be serv'd so, and by this Means reconcil'd them again. But it being probable that some People abroad may fall into the same Mistake, and so condemn the System, I desire you will publish this; for unless there be Provision made to keep out Insects, Plants may be impregnated by Insects much smaller than Bees.

J. S. Wahlbom, one of Linnaeus' students, referred briefly to insect pollination in his dissertation, *Sponsalia Plantarum* (1746), noting that "bees do more good than harm for they scatter pollen on the pistil" (*Amoen. Acad.*, Vol. I). Arthur Dobbs (1750) went into the subject of pollination in considerable detail (*Phil. Trans. Roy. Soc. London* 46:536–549). By the time Koelreuter discovered the importance of insect pollination, the fact that it occurred was widely known. But Koelreuter did stimulate the researches of Christian Konrad Sprengel (1750–1816), who investigated and described in admirable detail the structural adaptations of flowers to their insect visitors. We shall return to the work of Sprengel later.

At this point we shall consider the contributions made to biology by an informal group of active and talented amateurs. This group had no formal organization, but its members were united to each other by their common interests, interlocking friendships, and voluminous correspondence. Most of the members were English, if we include as English those who lived on both sides of the Atlantic, but some were Dutch or Swedish. The group was composed of men of many occupations.

First, there were the explorers, who were interested in the flora and fauna of the newly settled British Colonies. Some of these became well known. Here we will mention but four: John Clayton I (died 1728) was a clergyman who visited Virginia in 1685 and described the native birds, mammals, reptiles, and plants in articles published in the *Philosophical Transactions* (Vols. 17 and 18); John Lawson (died 1711) left England for Carolina in 1700 where, eleven years later, he was captured and killed by the Indians. He

published *A Voyage to Carolina* (1709), a book that was republished as *A History of Carolina* in 1714 and again in 1718; Mark Catesby (1680–1749) spent ten years in Virginia and Carolina where he studied the geology, anthropology, zoology, and botany of the region. He published the *Natural History of Carolina, Florida and the Bahama Islands* (1731–1743), a work whose elephant folio plates were engraved and colored by the author himself. In artistic merit, Catesby's drawings are second only to those of John James Audubon, whose work appeared a century later. Peter Kalm (1715–1779), of Åbo in Finland, was a student, friend, and travelling companion of Linnaeus. He journeyed through the northern American colonies and Canada during the years 1748 to 1751. He returned to Åbo where he served as a professor in the university. He published his famous *Flora Fennica* (1765) and his popular and important *Travels in North America* (1753–1761).

Some of the explorers and collectors were native-born Americans who sent their specimens to be described by the better-educated European botanists. John Bartram (1701–1777) and his son, William (1739–1823), were the most famous of these. They explored eastern America from Canada to Florida. William Bartram's *Travels through North and South Carolina, Georgia, Florida, etc.* (1791) was the perhaps chief factor in creating the eighteenth-century European fashionable attitude toward nature and toward the "noble savage." John Clayton II (1685–1773), the clerk of Gloucester County, collected extensively in Virginia, and sent his specimens to Jan Frederik Gronovius (1690–1762), a professor in Leiden, who published the *Flora Virginica, Exhibens Plantas, quas Nobilissimus vir D. Johannes Claytonius in Virginia crescentes observabit atque collegit., 1739–1762.* And finally, John Bannister (1650–1692) and Alexander Garden (1730–1791) made collections from the southern colonies and brought to light many new species.

An important center of botanical activity was furnished by the members of the group who were professional florists, nursery men, and gardeners. The most important of these were Thomas Fairchild (1667–1729), who experimented widely with plants and who wrote *The City Gardener* (1724); William Knowlton (1692–1782), who spoke several times before the Royal Society; and Philip Miller (1694–1771), whose observations of insect pollination we have cited earlier.

Also belonging in the group, and indeed a most important part

of it, were a number of gentlemen-amateurs; Sir Hans Sloane (1660–1753), President of the Royal Society, was the most famous of these. His was the chief role in establishing the British Museum. His *Voyage to the Islands of Madeira, Barbados, etc.* (1707–1725) and his *Catalogus Plantarum, etc.* (1696), describing the plants of Jamaica, established his scientific reputation. James Petiver (1663–1718) also built up a collection that went into the British Museum. Peter Collinson (1694–1768) was a Quaker merchant, whose contribution it was to distribute the plants that were imported into England, Patrick Blair (d. 1728) published a number of letters that were written by several members of the group in his *Botanick Essays* (1720), as did Richard Bradley, one-time professor at Cambridge, in his numerous books on husbandry and gardening.

Finally, our group of dilettanti included a number of the leaders and important citizens of the American Colonies. It included Cotton Mather (1663–1728), a Fellow of the Royal Society and a leading New England divine. He published many books that contained a number of incidental observations on natural history. Paul Dudley (1675–1751) was also a Fellow of the Royal Society, as well as Attorney General and a Judge of the Superior Court of Massachusetts. William Douglass (fl. 1760) was a distinguished physician and author who lived in Connecticut. Cadwallader Colden (1688–1776) described many new species of plants—often not very wisely. He was the Governor of New York. James Logan (1674–1751), the Governor of Pennsylvania, investigated the sexual reproduction in Indian corn and published his experiments in the *Philosophical Transactions* (1735) and later and more extensively in Leiden (*Experimenta et Meletamata de Plantarum Generatione,* 1739). William Byrd (1674–1744), President of the Council of Virginia, wrote a number of books that were not published until after his death. His *Neu-Gefunden Eden* was printed in Bern in 1737, but his authorship of this work was concealed by his use of the name "Wilhelm Vogel." His books that were published later contain many detailed accounts of the natural history of Virginia and Carolina.

When we examine this British-American group critically we are struck by the fact that few of its members made any contribution to biology important enough to be included in an elementary history of the subject. Their combined contributions, however, are important. Here we have an excellent example of how the science of

biology was beginning to grow. It was advancing through the cumulative effects of a great many minor steps.

As we know, no history of science is complete or even adequate if it lists and evaluates only the major advances made by a few great scientists. Unfortunately limits of space and time often make it necessary for the historian of science to skim over the accumulation of knowledge that comes through the activity of a whole covey of minor scientists. Yet the total of these minor steps constitute the major portion of our science.

The men in the group we have described introduced a great many useful and ornamental plants into Europe. They experimented in plant physiology and discovered the various ecological requirements of different types of plants. They increased enormously the number of plants known to science and proved the sexuality of plants by their experiments in plant hybridization. Of the thirty or more investigators who described plant hybridization before Koelreuter began his classical investigation, a round dozen belonged to this group.

Our first record of a hybrid plant is both casual and ambiguous. John Lawson stated in 1709, "Bastard-*Spanish* is an oak betwixt the Spanish and the red-oak. . . ." This is the kind of statement that we could dismiss very easily, were it not for the fact that the "Bastard-Spanish" is a naturally occurring hybrid between *Quercus falcata* and *Q. rubra*. But did Lawson know that it was a hybrid when he referred to it so casually? Here is the ambiguity: "bastard" meant hybrid in Germany, but not in Britain. When a British botanist called a plant a bastard, it meant only that he did not like the plant—he considered it inferior. We have evidence, though, that in the American Colonies, hybrid plants were sometimes known as "bastards." William Byrd quoted Lawson's statement and John Clayton II used "bastard" to mean "hybrid."

Our second record of a plant hybrid is not ambiguous, although its presentation may seem a trifle frivolous. It was in a letter written by Cotton Mather to James Petiver dated September 24, 1716. This letter was not considered important enough to publish at the time—indeed it was not published until the twentieth century—but Cotton Mather told the same story again in his *Religio Philosophical, or The Christian Philosopher* (1721). After describing the sex of plants and the role of pollen in wind fertilization, he wrote:

First my friend planted a row of Indian Corn that was Coloured Red and Blue; the rest of the Field being planted with corn of the yellow, which is the most usual colour. To the Windward side, this Red and Blue Row, so infected Three or Four whole Rows, as to communicate the same Colour unto them; and part of ye Fifth, and some of ye Sixth. But to the Leeward Side, no less than Seven or Eight Rows, had ye same Colour communicated unto them; and some small Impressions were made on those that were yet further off.

Secondly: The same Friend had his Garden ever now and then Robbed of the Squashes, which were growing there. To inflict a pretty little punishment on the Theeves, he planted some Guords among the Squashes, (which are in aspect very like 'em) at certain places which he distinguished with a private mark, that he might not be himself imposed upon. By this method, ye Thieves were deceived, & discovered, & ridiculed. But yet the honest man saved himself no Squashes by ye Trick; for they were so infected and Embittered by the Guords, that there was no eating of them.

Following Cotton Mather's description of plant hybridization, other descriptions came, many and fast. In 1717, Richard Bradley (*New Improvements in Planting and Gardening*) described how Thomas Fairchild had crossed a Carnation with a Sweet William (*Dianthus caryophillus* X *D. barbatus*), and how he thus produced the sterile "Fairchild's Mule," a plant that was propagated vegetatively for over a hundred years. In the same work Bradley described his own hybridization of *Auriculas*.

In 1721, Philip Miller described his *Brassica* hybrids in a letter to Patrick Blair dated October 19 (published in the *Philosophical Transactions,* Vol. 31), and three years later Paul Dudley described variety crossing in *Zea* (*Philosophical Transactions,* Vol. 33). In 1739, John Bartram described his *Lychnis* hybrid in a letter to William Byrd.

Peter Kalm (*Handlingar Kongl. Svenska Vetensk. Acad.*) described hybridization in *Zea,* in 1750, as did William Douglass (1751) in Volume II of his *A Summary Historical and Political of the First Planting,* etc. John Clayton II, William Knowlton, and Patrick Blair also wrote on plant hybridization and, inasmuch as Carolus Linnaeus and a number of his students did the same, the existence of plant hybrids was well known when Koelreuter undertook his classical experiments.

Between 1761 and 1797, Koelreuter published more on plant hybridization than had all of his predecessors combined, and he succeeded in placing the entire subject on a firm scientific foundation. We should mention also that his work on insect pollination stimulated the efforts of Sprengel.

Christian Konrad Sprengel (1750–1816) published in 1793 a work

of real historical importance, *Das entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen* (Figure 43). Sprengel had an eye for all that was beautiful in his environment. So was it to be wondered at then that he, who hitherto had had only a perfunctory knowledge of the collaboration between insects and flowers, felt as though he had entered fairyland? Through his concentrated attention and constant observations he gradually discovered the harmony between the structures of flowers and insects. He discovered their mutual accommodation, which seemingly had but a single aim: the pollination of the flowers!

Figure 43. Title page of Sprengel's *Das entdeckte Geheimniss* (1793).

Sprengel was wholly teleological in his conclusions, as he indicated at the beginning in his introduction: "Convinced that the wise Creator of Nature did not create one single hair without a definite purpose, I began to consider what might be the purpose which these hairs served." This may be a somewhat naive and childish attitude—Sprengel's nature-research was directed perhaps too much to a utilitarian aim—but nevertheless it was his work that laid the foundation for one of the most fascinating chapters in botany, that of the biology of the flower.

The significance of sexuality in plants was established chiefly by the work of three men: Camerarius, who proved the existence of sexuality in plants experimentally; Koelreuter, who convinced the biologists that it was possible to unite plants of different varieties into one hybrid; and Sprengel, who revealed the true nature of the pollination apparatus. The more detailed examination of the process of fertilization, and especially the accurate investigation of the sexuality of cryptogams, was left to a later period.

Yet, soon after Camerarius made his discovery, it was to be applied in a most unexpected manner: to a radical reform of the system of plant classification. Here we come to the great figure of Carolus Linnaeus (1707–1778; Figure 44). Since Ray's time, not much had been accomplished in this field. J. P. De Tournefort (1656–1708) had, in his *Institutiones Rei Herbariae* (1700–1703), added a few improvements to the system. To an even greater extent than Ray, he accepted the structure of the flower as the essential guide for classification. Hence he could give more precise definitions of the genera; but Tournefort's descriptions of species were still exceedingly primitive. Taken as a whole, his system was inferior even to Ray's in some important aspects.

If the system of plant classification around the year 1700 was not very satisfactory, neither was the classification used in zoology. Changes were, in fact, made from time to time, but no basis had been found which could be defended on principle. This was left to Linnaeus.

In his childhood, Linnaeus' father had taught him to love plants and animals; during his student years at Lund and Uppsala this love changed to a zest for study and research. On his travels to Lapland (1732) and through Sweden, this zest turned to passion. The study of classical biological literature, from Aristotle to Tournefort, led his burning curiosity into the paths of scientific investiga-

Figure 44. Carolus Linnaeus (1707–1778).

tion. Schemes of work grew abundantly in his brain. The writings of his predecessors satisfied him only partly; his critical and original mind grasped possibilities entirely different from any his predecessors had envisioned. In a letter dated October, 1733, written when he was only 26, he listed some thirteen works that he intended to write, most of which were soon to become realities. Already he had enough material at his disposal, but it was not until 1735 that he found the peace he needed to work out all the ideas that were seething in his mind.

Then came the turning-point in his life. He wanted to get mar-

ried, for he had found his ideal mate in the daughter of Dr. Johan Moraeus. He was already an excellent systematist, and he had become expert in evaluating the important attributes and characteristics of all kinds of organisms, including the human. He had stated several times his precise specifications for an ideal wife. First of all he wanted her to have a respectable dowry, second, she should have smooth, firm flesh, and finally, she should be of a supple disposition.

Sara Lisa Moraeus had these assets, but later on—in middle-age and married to Linnaeus—she became miserly and ill-tempered. When her husband died, she tried to sell his herbarium, which was the chief possession he had willed to their son—Linnaeus the Lesser. She desperately wanted more money for her daughters' dowries. When her son died, she succeeded in selling the herbarium to some Englishmen, who loaded it into a merchant vessel, which immediately set sail. The Swedish government tardily awoke and sent a warship to intercept the English merchantman, but it failed in its purpose. The herbarium reached England, where it became the proud possession around which the Linnaean Society of London was founded.

Dr. Moraeus refused to let Linnaeus marry his daughter until he had his doctorate, a degree he could not get in Sweden. So Linnaeus journeyed to the University of Harderwijk in the Netherlands, a university that was at the time something of a "diploma mill." The chief industries of Harderwijk were well known, and a song of the time went:

> Harderwijk is een stadje van negotie,
> Men koopt er bokking, blauwbessen en
> bullen van promotie.

which we can translate:

> Harderwijk is a town of trades,
> They sell kippers, blue-berries
> and University-grades.

Needless to say, the University of Harderwijk has never regretted giving Linnaeus a degree.

Linnaeus already had his dissertation written when he arrived at Harderwijk early in June, 1735. He received his degree on June 24, so his sojourn at his *alma mater* lasted only a few days.

Linnaeus had to go back to Sweden as soon as possible since

his fiancée was awaiting his return. But first he wanted to take the opportunity of meeting some famous people. He had prepared his own introduction; his first work of fundamental importance was completed. He had brought with him an article of fourteen folio pages, in which he set out a new classification of the three realms of nature: the plants, animals, and minerals. This treatise made such an impression on A. van Royen, then professor at Leiden, that he immediately helped Linnaeus to have it published. In this way there appeared what was virtually—except for a short publication on the flora of Lapland—his first book, *Systema Naturae* (1735).

This little work opened many doors for him. At the instance of van Royen, old Boerhaave was prepared to receive him. This visit led in turn to a recommendation to George Clifford, Mayor of Amsterdam and one of the chiefs of the East India Company. Clifford offered Linnaeus hospitality on his estate, "Hartecampe," near Haarlem. He wanted Linnaeus to be his private physician and he also had the idea that Linnaeus should describe his collection of plants, which had been assembled from various countries.

Linnaeus accepted this proposition with alacrity. The three years he was to spend at Hartecampe gave him the opportunity of complying with Clifford's wishes by writing three works, numbering 600 pages in all (*Musea Cliffortiana*, 1736; *Hortus Cliffortianus*, 1737; and *Viridarium Cliffortianum*, 1737). It also offered him suitable working conditions which, by enriching his experience and giving him time to ponder at leisure the data he had collected, enabled him to carry out the schemes drawn up in 1733. During these years he completed and published no fewer than six books in addition to the Clifford works: *Fundamenta Botanica* (Amst., 1736, 36 pp.), *Bibliotheca Botanica* (Amst., 1736, 153 pp.), *Flora Lapponica* (Amst., 1737, 372 pp.), *Genera Plantarum* (L.B., 1737, 384 pp.), *Critica Botanica* (L.B., 1737, 270 pp.) and *Classes Plantarum* (L.B., 1738, 656 pp.). This output, in addition to the little *Systema Naturae* and the three books about Clifford's collection, testify to Linnaeus's incredible capacity for work and his complete mastery over the material.

Meanwhile, he also found time and opportunity to perform a sacred duty: his best friend, the young zoologist Peter Artedi (1705–1735), who had died in the Netherlands, left behind him a work on fishes. Linnaeus undertook to prepare this for the press; it appeared in Leiden in 1738 (*Ichtologia*, 556 pp.).

The capacity and zest for work remained with Linnaeus all his life. His genius was soon recognized officially in Sweden. After his return, many larger works, botanical as well as zoological, followed: *Philosophia Botanica, Species Plantarum, Flora Suecica, Flora Zeylanica, Fauna Sueciae Regni, Materia Medica Regni Vegetabilis, Materia Medica Regni Animalis* and, in addition to these, an impressive series of shorter treatises in the ten volumes of the *Amoenitates Academicae*. He furthermore prepared successive reprints and new editions of his previous works. The original *Systema Naturae* of fourteen folio pages grew in the twelfth—the final version edited by him—into a work of three volumes, numbering 2300 pages.

Because of the fundamental nature of their contents the *Systema Naturae* and the *Philosophia Botanica* are his most important works. In these, his principles found their clearest expression. Seen superficially, it was his striving towards exactitude, and his logical constructions of the plant and animal systems which are the most striking. This tendency for systematizing was and still is ridiculed at times. Indeed Linnaeus, as a compiler of systems, cannot be entirely exonerated from an occasional exaggeration.

Another factor, however, comes to the fore when we examine his opinions more closely. His predecessors, both botanists and zoologists, aimed at gathering all the known forms of organisms together into one system so that they could survey the whole field at a glance. Linnaeus realized that systematists do not aim merely at the practical goal of a classification, but that their work has a far wider scope.

Classification has a scientific background and its problems are scientific. Also, the problems of animal and of plant classification are essentially alike. Previous authors had begun at the top: they divided all that was known into large groups. They descended from these to the smaller groups and finally reached all the way down to the species. Linnaeus started with the species, grouping the more or less similar species together to form genera, analogous genera to form families, and related families to form orders and classes. He held that the concept of species must form the basis for every system.

The ancient taxonomists began with a more or less vaguely suspected natural classification, but as the evidence for such a system soon dwindled, they resorted to all manner of artificial arguments to

justify their groupings. The logical deductions of Linnaeus on the other hand led him to draw a distinction between an artificial and a natural system, the artificial system being a necessary evil, the natural an ideal hard to attain.

For the time being, then, he had to restrict himself to an artificial classification. With the intuition of genius, he chose the structure of the reproductive organs of the flower as a basis for grouping the higher plants. This was after he had learned indirectly of the importance of the stamens, an importance that was proven by Camerarius. After distinguishing between plants with real flowers and seeds (phanerogams) and plants without real flowers or seeds (cryptogams), he subdivided the former into hermaphrodite flowers (and these again according to whether the anthers were compound or separate, and according to their numbers) and plants whose flowers were at least partly unisexual. In this way his famous *sexual system of plants* came into being. For a natural classification—which was to be the "primum et ultimum" of botany—he had as yet only fragmentary data.

Linnaeus realized that the basis for this natural classification lay in a system of clearly defined *species*, not as logical abstractions, but as natural units. In his eyes the species was a concrete object existing at all times and invariable. He held that there were just as many species as the Infinite Being had created in the beginning. Exterior influences such as climate, soil, temperature, and wind might change individual appearance thus causing a "varietas," but these were of no importance to the botanist. Later he began to doubt the absolute constancy of the species and he changed his notion of "in the beginning" to the more general idea of "in principle." He went a step further and held that there were as many genera as there were different types of floral structure shown by "natural" species.

His sexual system naturally was not useful for classifying animals. He used the teeth and the toes as the basic characteristics in mammals; and the shape of the beak in birds. At first, as in the original edition of *Systema Naturae,* he gave only the names of genera. In later editions (notably the twelfth) and in special botanical works, he expanded these generic names with short diagnoses. He tried to be unprejudiced in his search for natural genera and to derive each one from a "diagnosis" of the genus.

The overwhelming importance of Linnaeus' work in the estab-

lishment of a soundly-built classification of plants and animals, gave rise to the expression: "Deus creavit, Linnaeus disposuit"—"God created, but it was Linnaeus who put all things in order." This caused him to be regarded all too often as a one-sided classifier. Although classification is, in fact, the predominating subject of his main works, his smaller treatises show how constantly he was aware of other biological problems.

His *Amoenitates Academicae* especially were full of such subjects. These ten volumes contain a series of so-called "theses" by pupils, whose substance and content were derived, however, almost wholly from the teacher. The teacher's pen also had an important share in their composition. They may rightly be looked upon as Linnaeus' spiritual property. The vast majority of these "theses" were concerned with a single species of plant or with the flora of a specific region, but there were some too, that dealt with biological subjects that have little or no connection with classification.

It is obvious that the sexuality of plants occupied a prominent place in these publications: the treatise called *Ficus* (1744) had already recorded the correct observations about the extraordinary pollination and setting of the fig. *Sponsalia Plantarum* (1746) was a critical survey of all that had hitherto been written about the sexuality of plants. Thereto Linnaeus added his own observations, as, for example, an experiment he performed at the age of 16 with a calabash plant, proving that the female flowers formed no fruit after the male flowers had been removed.

Linnaeus also knew of the existence of plant hybrids. He looked upon *Peloria* (1744), a strikingly anomalous form of the toad-flax, as a hybrid. In a separate essay, *Plantae Hybridae* (1751), his pupil, Johannes Haartman, related all he knew about hybrid plants. In 1759 Linnaeus competed for a prize to be awarded by the Petersburg Academy, with an article entitled *Disquisitio de Sexu Plantarum*. Here he described several hybrids found in nature and one or two that he had produced artificially. He investigated the biology of flowers and, noticing that they opened and closed at specific times of the day, he even went so far as to draw up a "plant-clock" (*Somnus Plantarum*, 1755).

Further treatises were devoted to morphological questions: the parallel between leaves and the parts of flowers, and between sepals, petals, stamens and carpels, were treated by him in great detail in *De Prolepsi Plantarum* (1763). Yet, none of these questions was

related to the principal aim towards which Linnaeus strove with such incredible concentration. His aim continued to be, simply, the construction of a system of classification not only for plants, but also for animals. He strove to connect the attempts at classification, which had been hitherto accepted as an unfortunate necessity, into an independent science. It was this which deservedly confers upon Linnaeus the title, bestowed on him during his lifetime, of "princeps botanicorum"—"prince of botanists."

Research into the vital processes of plants is rarely possible by means of observation alone. Camerarius and Koelreuter had shown this by their experiments on pollination and hybridization; but they were able to conduct their experiments without any technical aids. A closer investigation of the other processes of life, such as feeding and respiration, becomes possible only by utilizing specialized methods. By the eighteenth century, the period of contemplation and observation was passing, and a new period was beginning. At this time Stephen Hales (1677–1761) realized that physiological activities may, in part, be considered as physical processes and that, to describe them properly, a physical technique should be used—a technique of calculating, measuring, and weighing.

The realization that vital processes might be in part physical (or chemical) raised the question as to whether it was possible that all vital activities might not be physical and, if they were, would there then be any need to call upon any super-physical or supernatural force to explain the behavior of living things? Inevitably the question would be answered both in the positive and in the negative. Perhaps the most striking answer was given by Julien Offray de La Mettrie (1709–1751).

La Mettrie was brought up to be a priest, but as a young theologian he became interested in medicine. Having studied medicine at Paris and at Leiden, he developed into a highly successful practitioner. However, he published a work, *L'Histoire Naturelle de l'Ame,* that aroused the opposition of the theologians. For safety, he fled from France to the Netherlands. Once there, he wrote his famous *L'Homme Machine.* This book was too much, even for the Dutch, so he fled once more, this time to the more tolerant Prussian court of Frederick the Great.

La Mettrie could be described as a complete mechanist and

materialist. Anything that resembled or corresponded with the immortal soul of the theologians, he held, simply did not exist. He sought to discard all the systems of moral and ethical standards based upon religious beliefs and undertook to erect in their stead a system founded upon nature. Our natural instincts and desires, he held, served as an adequate guide to the virtuous life. Nature, far from being evil, was the supreme and only good, and, consequently, good and healthy conduct followed naturally from doing what was natural.

Of course, La Mettrie missed the fact that our doing what was pleasant would be the same as doing what was "good" only if our species were *completely* adapted to its surroundings—only if it were one hundred percent fit. And the absolute fitness of a species, we know, is too rare to be of any consequence. It also implies a complete cessation of evolution. Perhaps, such an organism as the horseshoe crab (*Limulus*), which still seems to be holding its own although it has not changed noticeably during the past two hundred million years, could be completely fit and could follow all of its desires without danger. Perhaps only *Limulus* could live as a complete Epicurean without inhibitions and without biological sin. La Mettrie, himself, could not. At the age of forty-two, he stuffed himself on a truffle pastry, and died in great discomfort. His death rather than his life makes him unique among biologists and his place in the history of science secure. Thus far, he is the only biologist on record who died from eating too many truffles.

Along with Hales, the period of the eighteenth-century physiologists was one of transition between such classical physicists as Robert Boyle (1627–1691) and Isaac Newton (1643–1727), and the rise of chemistry, which was due to the work of Joseph Black (1728–1799), Henry Cavendish (1731–1810), Joseph Priestley (1733–1804), K. W. Scheele (1742–1786), and A. L. Lavoisier (1743–1794). It was the epoch in which G. E. Stahl (1660–1724) formulated his "phlogiston" theory. According to this theory every inflammable substance was said to contain a certain constituent, phlogiston, which was given off when it burned. It was through the influence of Boyle and Newton that Hales managed to evade the suggestive power of this theory. He may justly be regarded as the forerunner of the coming revolutionary advances in chemistry. Hales' great achievement was that he realized the unsatisfactory

character of Malpighi's and Ray's Aristotelian physiological theories. Consequently he attached more value to the opinions of the physicist E. Mariotte (1620–1684).

The Aristotelian school had taught that such different chemical characteristics of plants as their taste and smell were dependent on different tasting or smelling "elements" derived from the soil. Mariotte defended a different view in his *Sur le Sujet des Plantes* (1679), namely that each plant absorbs the same substances (salt, nitre, sulphur, earth, and water) and is able at will to manufacture out of these its individual gustatory and olfactory substances.

Hales developed this concept as far as his contemporary chemistry allowed him to, but the time for a chemical physiology of plants had not yet arrived. With the aid of simple instruments he examined the transpiration of plants, the pressure exercised by the roots in drawing water up from the soil, and the suction contributed by the leaves (*Vegetable Staticks*, 1727). He studied the force by means of which peas, soaked in water, could crack the glass in which they were enclosed.

Hales removed a ring of bark and noticed that the edge of bark above the ring grew much thicker than the lower edge. He suspected that plants took some of their nourishment in from the air and set out to verify this by an "Attempt to analyse the air, by a great variety of chymio-statical experiments." In the course of these he managed, *inter alia,* to liberate "air" (oxygen) from red lead, thus becoming a forerunner of Priestley. He understood light to be an essential factor in plant life, for in a dense forest the lower branches do not get enough light, "and drawing little nourishment, they perish." He invented an instrument for marking a growing part of a plant—the stalk or the leaf—in order to measure the growth of a given part day by day. In view of all this we may consider Hales to be the founder of *physical plant physiology.*

The chemical problems in this field were still difficult of access; for this, it was necessary to wait for the achievements of eighteenth-century chemists. The first step was the overthrow of the phlogiston theory through the discovery of oxygen by Priestley and Scheele and the correct interpretation of this discovery by Lavoisier. (Priestley, however, remained committed to the phlogiston theory.) It was on these discoveries, that Jan Ingenhousz (1730–1799), Jean Senebier (1742–1809), and N. Th. de Saussure (1767–1845) continued to build. The first discovery was that the green "substances"

appearing in the water were able to restore "vitiated" air and render it once more suitable for respiration and combustion. This discovery was derived from the physicist Priestley (1772). Priestley, however, did not identify the green substances as algae, and he was still too much involved in the phlogiston theory to realize that the restoration of vitiated air was the production of oxygen by plants.

A few years later (1779), Ingenhousz supplemented Priestley's description with the information that all green plants had this power when they were placed in the light, that the power was diminished if they were placed in the shade, that it disappeared in the dark, and that, in the dark, the plants polluted the air just as animals did. But it seems to have been impossible for Ingenhousz to assess this process in relation to Lavoisier's conception of the true nature of oxygen.

Nevertheless, we may attribute to Ingenhousz the discovery of the assimilation of carbon dioxide as a process independent of the production and giving out of carbon dioxide, the more so when, in a second work, *An Essay on the Food of Plants* (1796), he presented and explained his observations in their true light. Meanwhile, Senebier also had worked along these lines in his *Recherches sur l'Influence de la Lumière Solaire, pour Métamorphoser l'Air Fixe en Air pur par la Végétation*, (1783). It is hard to tell who was the first to discover oxygen assimilation, because of the complex history and the diffuse language of Senebier. They are best considered together as the eighteenth-century founders of chemical plant physiology.

Closely following in their footsteps came N. Th. de Saussure, who rendered valuable service in attempting to describe more accurately the processes of assimilation and respiration, which had only been sketched in rough outline by Ingenhousz and Senebier. Saussure laid down quantitative definitions and worked out from these the ratio between the assimilated oxygen, the carbonic acid given off, and the resulting asset to the plant. He extended his chemical research to the nourishment of plants—that is, he proved that the elements of water are also assimilated and that, in addition to carbonic acid, nitrogen compounds and mineral salts were also essential to the plant's normal growth.

Thus we see that plant physiology made great advances at regular intervals of about fifty years: Mariotte in 1679, Hales in 1727, and Ingenhousz in 1779.

The last decade of the eighteenth century is notable for several widely divergent discoveries and ideas, whose significance was not realized at the time and whose importance is only now beginning to be understood. We have already called attention to Sprengel's work of 1793, in which he described the complex structural adaptations of flowers to insect pollination.

The year before Sprengel's book appeared, François Huber published a collection of letters under the title of *Nouvelles Observations sur les Abeilles* (1792). Here he recorded the fact that the first eggs laid by the queen bee would hatch into drones if her nuptial flight had been delayed, and that her last eggs would also hatch into drones, presumably when she had used up her supply of spermatozoa. He also noted that sometimes the worker bees would lay eggs that could not have been fertilized and that they also would develop only into drones. He thus anticipated by more than half a century the discovery made by Johann Dzierzon (*J. Eichstadt Bienenzeitung*, Vol. I, p. 113, [1845]) that the drones came from unfertilized eggs, the queen and the workers from fertilized eggs.

The third major work of the decade appeared in 1794, when Erasmus Darwin, the grandfather of Charles Darwin, published *Zoonomia, or the Laws of Organic Life*. Here he described the cumulative effects of the inheritance of acquired characters and the subsequent and consequent alteration of species, thus anticipating by eight years Lamarck's endorsement and explanation of evolution. Later, in his *Temple of Nature* (1803), he recorded the struggle for existence in gory detail. Incidentally, no one has yet succeeded in determining just how much Charles Darwin owed to his grandfather. Darwin himself thought that he owed but little. The question, however, is still open.

The fourth—and last—work in this decade that we shall cite is a paper published in 1799 in the *Philosophical Transactions,* entitled "An account of some experiments on the fecundation of vegetables," by Thomas Andrew Knight. Knight hybridized a white variety of pea (*Pisum*) with the pollen from a gray variety and reported that all the hybrids were gray (the dominant character according to Mendel, sixty-six years later). He also back-crossed the hybrids to the white (recessive parent and reported that he got a number of different types, including the white. A quarter of a century later, in 1824, Knight, along with John Goss and Alexander Seton (*Trans. Hort. Soc. London*), would carry these experiments much further.

But the full significance of their observations would not be understood until the twentieth century—until after Gregor Mendel's forgotten work of 1865 had been recovered.

Biology, which had been cultivated more or less as a whole by each scientist up to the eighteenth century, began at last to show signs of differentiation into several sub-sections. Toward the end of the century, plant classification, plant anatomy, the comparative anatomy of animals, plant physiology, and animal physiology each began to have its own investigators. The highly specialized types of research required by the different fields made it necessary for a scientist to concentrate his entire faculties on one line of research and to specialize in one direction only. This specialization increased rapidly during the following decades.

10

Specialization

By the beginning of the nineteenth century, the biologists had collected such a store of verifiable information that its sheer mass and variety presented them with new and unusual problems. The science as a whole was becoming more complex and much more difficult to master. It now required a much longer time for the beginning student to reach the frontiers of knowledge, and it was becoming almost impossible for anyone to explore the entire boundary between the known and the unknown. The biologists, however, did what they could—they followed the only possible course of action. Most of them concentrated their interests and their attention, investigating as intensively as their intellectual equipment would permit some one small segment of the greater field. In so doing they succeeded in adding many important details to the ever growing store of knowledge. Of necessity, they had become specialists.

Specialization had become the order of the day, and while this was limiting in many ways, it was not all bad. By dividing up the task of discovery the biologists succeeded in advancing their science at an ever increasing speed; but, the further the science advanced, the more rigid became the requirements of specialization. The value of this specialization, which occurred, incidentally, in other sciences, is shown by the fact that it ushered in our modern age of science.

We may decry specialization and lament the intellectual limitation it imposes upon the specialists, but any honest evaluation of the growth of science will have to conclude that, without specialization,

the progress of science would soon come to an end. Modern science could never have come into being without that great mass of knowledge that only the labor of specialists could create. Literally thousands of important contributions were made to biology during the nineteenth century. Biological information was increasing exponentially, and this rate of increase it still maintains.

It was soon evident that the scientists needed special aids if they were to keep up with themselves and not be so lost in a growing wilderness of facts that they would unwittingly labor to discover what was already known. They needed aids that would tell them what their colleagues were doing and what their predecessors had done. The Royal Society came to the rescue with its *Catalogue of Scientific Papers*, published in nineteen volumes from 1863 to 1925. But this aid broke down completely when the twentieth century dawned. No such catalogue is possible today.

At present, about one million scientific papers are published each year, perhaps a hundred and fifty thousand in biology alone. *Biological Abstracts* now prints some 100,000 abstracts a year. But the number of contributions continues to increase. Later on we shall consider just where our voluminous activities may lead. Here we shall merely call attention to the hazards that lie in the very affluence of our science. There is real danger that, if all science continues to grow as it does now, sometime in the near future it may become smothered in its own fat.

At the beginning of the nineteenth century, however, the full effects of specialization were not felt. The specialists were not completely specialized. Their background information was still broad and relatively complete. Some of the better-informed still had a store of knowledge that encompassed the entire science. The limitations that even the more productive biologists had lay in their specialized interests and in their contracted fields of research, not in their background information. We should have to await the twentieth century to find professional biologists who were ignorant of the greater part of biology. Fortunately, some of the abler biologists of the nineteenth century were able to transcend the limitations of their technical research and, as they matured, to turn their attentions to the theoretical basis of the whole of biology.

These were the men who brought about the revolutionary reorientation that took place about the middle of the century. These were the men who changed our entire outlook on the universe and

made us re-evaluate even our own species. These were the biologists whose names are still familiar to the educated world. It is well for us to emphasize, however, that these famous men were also specialists, even though their specialized accomplishments form but a small part of their merit.

Almost every educated person can recognize the names of the men who created the theory of organic evolution and who established it on a firm scientific basis. But it may come as a surprise to many to learn that Charles Darwin's standing as a biologist came originally from his work on coral islands and on the fossil cirrupede Crustaceae. Few would know that Thomas Henry Huxley reached the highest ranks of his profession, while still a very young man, through his contribution to our understanding of the *Coelenterata*, and the *Cephalous Molusca*. Ernst Haeckel was the world authority on the *Radiolaria* and the *Siphonaptera*. August Weismann did distinguished work on the *Diptera* until his failing eyesight forced him to turn to more theoretical problems. Hugo de Vries was the plant physiologist of repute who gave us our first extensive knowledge of the osmotic pressure within the plant cell. Incidentally, it was the reputation of these men as competent naturalists that made their support of evolution so effective. Without their support and the support of others like them, the acceptance of the theory of evolution might have been delayed indefinitely.

The nineteenth-century specialists, however, who were nothing but specialists, are now beginning to fade from our memory, vanishing into the vast but undistinguished mass of our anonymous ancestors. For us to acknowledge our debt to these specialists individually is hardly feasible—certainly we could not do so in a history of biology that anyone could ever finish reading. Here we can express only our appreciation to them *en masse*, and distinguish by name only the outstanding few who were the acknowledged leaders of their day.

Of course, the contributions made by most of the specialists were trivial—then as now—but the sum total of their accomplishments was far from trivial. Today, we know that their work has given us the foundation on which all future advances will be built. Their detailed discoveries constitute the factual background which all our incipient biologists have to assimilate before they can become professionals—or even before they can become effective journeymen.

Without this solid, detailed, accurate, and organized mass of facts, our science could not exist.

As we have stated, during the nineteenth century, the biologists uncovered a tremendous number of facts. But facts in themselves have no meaning. The significance of a fact lies in its relation with other facts and, as we know, it is the primary function of science to bring out this relationship—to organize all the given facts, i.e., data —into some reasonable and understandable framework or hypothesis.

Early in the nineteenth century, a number of the hypotheses on which the biologists were relying proved to be inadequate. They could no longer accommodate all of the new discoveries. Some of the most important hypotheses had no unoccupied niches into which the newly acquired data could fit. Sooner or later all such hypotheses—even the most respectable ones—would have to be scrapped, and this discarding of ancient beliefs has always been a most painful procedure. And some of the biologists showed their pain! Some resisted the changes, but others did not, and this meant widespread disagreement. A number of the controversies that arose were violent, bitter, and very personal.

Few outworn ideas can be discarded without controversy but, in spite of all such disturbances, it was inevitable that biology should experience some revolutionary alterations, and that ultimately, the biologists should have to be re-oriented toward their more basic assumptions. As the science expanded, it grew in stature and extended its field of coverage. It also became more intimate and personal as it began to engulf the biologists themselves. It took as its field more than the mere structure and functioning of their animal bodies. The biologists had to admit that, whatever else they were, they were also biological specimens. They were animals whose origin, existence, and fate were biological problems. Thus biology found itself invading fields that hitherto had been pre-empted by other disciplines. The theory of evolution alone brought biology into conflict with many religious and political doctrines, and the controversies that arose from this conflict were not confined to the biologists.

In recounting the history of our science during this period of *Sturm und Drang*, we shall have to be somewhat arbitrary, not only in regard to the subjects we emphasize but also in the organization of our presentation. In this chapter we shall be concerned chiefly

with the technical contributions made by the many-talented specialists who were so active during the period, and reserve for the following chapter our account of the great changes that took place in fundamental biological theory.

During the past few centuries, the working scheme of biology had undergone a basic alteration. It no longer started out from a philosophical edifice from which it could survey the entire kingdom of living things. Now, at the beginning of the nineteenth century, it rested upon a different fundament. Its foundations were lower but much firmer. Its methods of growth had also changed. No longer could it advance by a re-arrangement or a re-organization of the existing data. It needed new data and, to obtain these gifts, it had to experiment, and to devise new techniques. And then it had to weigh and compare the results of the experiments.

As we have stated earlier, biology had reached a stage in its development when no single individual could hope to master the whole. The multiplicity of new problems, however, assured ample ground for the full attention of all those who were interested in research.

It was not only the newer techniques of experimentation that required personal skill and adequate training. A special training was also needed for making accurate and adequate observations and for interpreting the ever increasing mass of material the biologists had to master. A broad outlook and a considerable insight were also required if the objective was to secure reliable information. Not much practice is needed to distinguish a horse from a cow—our television "westerns" have made the difference clear to almost everyone—but some preliminary study is needed to distinguish between different breeds of cows or to describe their various characteristics accurately. Moreover, if an observer wishes to distinguish between the individuals of the same breed, he has to have a still greater fund of knowledge at his disposal. It is essential also that an experimental scientist have a complete command of his research techniques when he attempts to learn anything beyond the trivial, or when he applies reagents to living material or manipulates his material with instruments.

The first parting of the ways came when plants and animals were investigated separately. The time when a Malpighi could compile both an anatomy of plants and an anatomy of insects was past. The divorce between botany and zoology was becoming complete. The eighteenth century had already shown signs of this separation of the

biological sciences into these two great spheres, and their division continued into the nineteenth.

The living microscopic organisms, however, which had such an attraction for a man like Leeuwenhoek and which did not fit completely into either botany or zoology were relegated temporarily into the background but, as the century advanced, they emerged into a division of their own. The science of microbiology was born.

The true beginnings of microbiology were much earlier, but it had a long gestation period. Perhaps we can date its conception as of the year 1676, the year in which Leeuwenhoek published his famous discovery of bacteria. It quickened noticeably in 1786 with the publication of a Dane, O. F. Müller, entitled *Animalcula Infusoria*. In a sense, this work ran parallel to that of Ledermüller's, but it surpassed Ledermüller's both in the superiority of its scientific treatment and in its more lucid and accurate descriptions. It attempted to define the species with generic and specific names, and thus it brought the micro-organisms into the same type of system as that devised by Linnaeus. Müller's conception of infusoria was still very broad, extending far beyond the present confines of the group. In addition to the actual infusoria, his "Infusoria" included protozoa, bacteria and other microscopic organisms. His work laid the foundations for a scientific system of Leeuwenhoek's "little animals."

From this eighteenth-century beginning, it was possible to continue the investigation of micro-organisms. In 1833, C. G. Ehrenberg opened up new possibilities with his *Die Infusions-thierchen als Vollkommene Organismen*. He discovered that "infusion-animals" have their own typical structure just as the higher plants and animals have. It cannot be denied that he had a creative imagination, however, when he endowed his infusions with musculature, a nervous-system, a vascular-system, and with digestive and reproductive organs. He did observe how some types absorbed their food and he believed, correctly, that they had their own metabolic processes.

Certainly, Ehrenberg's interpretations went too far, but he did lead the research on micro-organisms into important new physical channels, that is, into a study of their vital functions. But he did more than this: he proved that whole layers of soil owe their origin to these tiny beings, whose skeletons of silica or of calcium carbonate remain behind after they die, both in fresh water and in the sea. When they die they sink to the bottom where their skeletons are metamorphosed into rock formations.

The gate opened by Ehrenberg admitted those who followed him into the domain of the physiology of micro-organisms. In this region they could glimpse perspectives of exceptional interest, such as the part played by protozoa and microbes in the circulation of matter in nature, and in the role of micro-organisms as human and animal parasites. In the technical sciences also, the micro-organisms proved to be so overwhelmingly important that the existence of microbiology as an independent science has been amply justified. The micro-organisms play their multifarious roles (1) in the circulation of organic and inorganic substances in the soil, (2) in the most divergent industrial techniques, (3) in the normal digestive system of all higher animals, and (4) in infectious diseases of plants and animals. Louis Pasteur (1822–1895) and his contemporaries, Sir Joseph Lister, F. Cohn, H. A. De Bary, Robert Koch, J. E. Metschnikoff, M. W. Beyerinck, F. R. Schaudinn, and many others both great and near great have built up microbiology into an entity—into a science in its own right. Naturally, this science is not without its own internal differentiation—its own sub-divisions and specialties—but it exists as a unified and well-organized whole.

The division of the higher organisms into the fields of zoology and botany became increasingly clear as the century progressed. Within the framework of each of these sciences, the process of differentiation continued. Again and again new centers of interest appeared, centers round which a new section of biology could crystallize, just as in a concentrated solution small particles split off from larger complexes, disperse, and function as independent centers of condensation. Sometimes the initial stimulus was a single observation, which would act as a sphere of attraction; i.e., if it showed some logical connections with other observations or with other objects. In many cases such a sphere of attraction would be followed by experimental research and, to follow our simile, the complex of crystals would again be augmented.

Some of the subdivisions of zoology, such as morphology—the study of the external structures of animals—and anatomy—the study of the internal position and structure of individual organs—could not really be separated; they had become too interwoven. The primary method used in developing both fields was by description, founded on observation. This led at first to an accumulation of facts. But, by a comparison of analogous organs in different species or of different organs in the same species, theoretical conclusions of great

importance were reached. These conclusions could be tested and elaborated by means of experimentation.

Koyter and Fabricius were the first to create something in the nature of a comparative anatomy of the higher animals. Camper, Hunter, and Vicq d'Azyr, each in his own way, continued this line of development. It was left to the genius of the poet and natural-philosopher Johann Wolfgang von Goethe (1749–1832), however, and to the comprehensive understanding of Georges Cuvier (1769–1832), to place comparative anatomy on a theoretical and scientific footing, and thus to link together the most divergent regions of anatomical research. In doing this, Goethe and Cuvier prevented the further fragmentation of the science that seemed imminent at the time.

In 1795, Goethe published his *Erster Entwurf einer allgemeinen Einleitung in die vergleichende Anatomie, ausgehend von der Osteologie,* in which he established the concept of "types"—of a plan, or "norm"—to which all kinds of animals could be fitted and classified, thus providing a scheme in which every animal could be included and described. Cuvier, on his part, was a man who surveyed the entire field of comparative anatomy with a wide yet highly selective vision. But, in so doing, he showed that he possessed a remarkable talent for overlooking all the unwelcomed details that might have worried him. He was an eminent and influential figure, who, outside the sphere in which he had rightly grown famous, held rigidly reactionary and dogmatic opinions. He defended a number of opinions which could not withstand the corrosion of time.

In the same year, 1795, Cuvier expressed thoughts similar to those of Goethe, although they were still somewhat vague. In his *Mémoire sur la structure interne et externe et sur les affinités des animaux auxquels on a donné le nom de Vers* (1795) he explained at great length that nature, in constructing the animal body, always made a choice out of a very small number of preconceived plans. In 1800 and 1805 he followed this up with *Leçone d'Anatomie Comparée* in five volumes, in which he developed his theory of types far more extensively than Goethe had. Besides the "plan" of worms, Cuvier had become familiar with other types. He held that there were four main "plans" or types, those of the vertebrates, the mollusks, the articulate, and the radiate animals. He transferred this principle of organization from comparative anatomy to classification, thus placing this latter science on a more exact and theoretically correct basis.

The establishment of comparative anatomy proved that even anatomical data, ostensibly of little promise, might lead by means of the comparative method to problems and conclusions of a very general nature. In France, Cuvier's spiritual offspring received the blessings of Étienne Geoffroy St. Hilaire, and the further development of his ideas was undertaken by H. Milne Edwards and H. de Lacaze-Duthiers. In England, Cuvier's ideas were adopted by R. Owen and T. H. Huxley; in Germany by J. F. Meckel, M. H. Rathke, and K. Gegenbaur.

Between Cuvier and Geoffroy St. Hilaire, there was a marked contrast—a contrast that was not merely one of personality. Their "anatomie comparée" was also directed towards different ends. Cuvier had taken a broad view coupled with a certain audacity, and had aimed at applying it to systematics. Geoffroy St. Hilaire somewhat diffidently aimed at applying it merely to specific details, which he hoped would lead to an understanding of their functions. Cuvier's doctrine of the prototype disappeared altogether from comparative anatomy, but it did not vanish completely. It shifted over to the field of classification.

Geoffroy St. Hilaire's conception of the homology of organs or parts of organs, however, held its own and was destined to become one of the most important elements of comparative anatomy. This was due in part to the clear-sightedness of Owen (*Lectures on Invertebrate Animals*, 1843) who drew sharp antitheses between *analogy* and *homology*. He applied the first term to organs alike in function though not in structure, and the second to organs similar in structure in spite of a diversity in their functions. Comparative anatomy thus developed into a completely independent discipline, although its influence was felt in several other subdivisions of biology.

On the experimental side, the data collected by anatomical investigation was clarified considerably by experimental morphology, a field in which Trembley had already established the principles but which had lain dormant since his time. With the arrival of Wilhelm Roux, however, experimental morphology acquired new life, because Roux, like a prophet of genius, linked anatomy and embryology together, first in his *Einleitung zu den Beiträgen zur Entwicklungsmechanik des Embryo* (1885) and later in his address on *Die Entwicklungsmechanik der Organismen, eine anatomische Wissenschàft der Zukunft* (1889). These disciplines had been

merely descriptive until he related them to each other on an experimental basis.

Roux had a keenly critical mind, great originality, a talent for organization, and the scientific insight and courage to defend his convictions against powerful opponents. Thanks to his rare combination of gifts, he overcame a great deal of opposition, both passive and active. It is to him that we owe the study of developmental processes or developmental physiology. We are also indebted to him essentially for experimental morphology, anatomy, and embryology, disciplines that have given us a scientific background for the anatomical observations of the embryo. He created a new school and advanced markedly the science which, in the last decades of the century, went from triumph to triumph. In the first half of the present century, its continued progress was due both to the work of Roux himself and to that of his innumerable pupils (*inter alia*, Spemann) and followers.

Anatomy, the dissection of the organism into its constituent parts, is capable of being elaborated in both space and time—both statically and dynamically. Its spatial extension involves a penetration into the tissues which compose the organs, thus forming the cornerstone for the science of tissues—the science of *histology*. Its extension in time covers the whole development of the individual, from its origin as a fertilized egg cell to an adult complete with all its organs and organic systems. This temporal extension became the science of *embryology*.

Swammerdam had labored in the field that was to become histology when he studied the insects and other lower animals. Here the structure of organs and tissues was interwoven much more than in the higher animals. The further development of histology as a special study came as a result of the titanic labors of Marie François Xavier Bichat (1771–1801; Figure 45), who, in the thirty years of his short life, managed to build it up into a separate science. His phenomenal capacity for work, his compelling urge to do research, and the accuracy of his observations resulted in three impressive books, *Traité des Membranes en Générale* (1800), *Recherches Physiologiques sur la Vie et la Mort* (1800), and his *Anatomie Générale Appliquée à la Physiologie et à la Médecine* (1801). At the age of thirty, these books made him an important figure in biology.

Since Bichat, histology has acquired a more highly specialized technique and it has extended its scope. His followers, of whom the

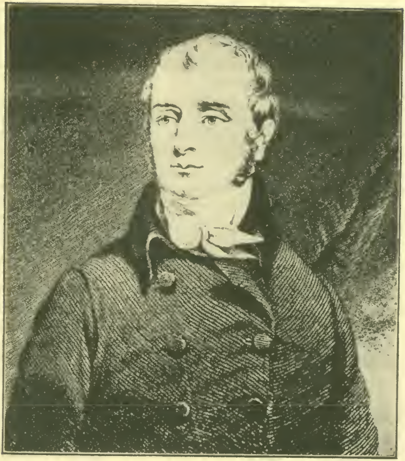


Figure 45. Marie Francois Xavier Bichat (1771–1801).

greatest were R. A. von Koelliker, F. Leydig, and S. Ramon y Cajal, have enlarged it further; but at the same time it has been their care to see that it did not become isolated. On the one hand, they brought it into close contact with *physiology*, which kept it an experimental science, and on the other hand with *pathology*, which made it possible to use its applied biological methods in the study of the abnormal tissue formations that occur during illness, thus making it a major adjunct to medicine.

The extension of anatomical data in time, the origin of the structures observed by anatomy, fall within the domain of embryology. Preliminary contributions had been made to embryology by Koyter, Fabricius, Harvey, Malpighi, and C. F. Wolff, but the science had

to await its transformation into a comparative science before it could attain its full importance. In this course of development it became closely connected with comparative anatomy: in fact J. F. Meckel, who took such an active part in promoting the latter science, also became the founder of comparative embryology. As he became familiar with the embryological development of different types of animals, the extraordinary parallel between the embryonic stages of higher animals and the adult forms of certain lower animals came to him as a veritable revelation. His *Entwürfe einer Darstellung der zwischen dem Embryozustande der höheren Tiere und dem Permanenten der niederen stattfindenden Parallele* (1811) laid the first stone for what was later to be formulated by Ernst Haeckel as the "biogenetic law."

This discovery of Meckel's had the effect of an electric spark on the younger of his contemporaries, in particular on Karl Ernst von Baer (1792–1876 [Figure 46] and C. H. Pander, 1794–1865), two friends, of whom Pander was probably the most original and von Baer the most energetic and persevering. Together these two rank as the pioneers of modern morphological embryology. By von Baer's discovery of the egg cell in mammals (in which he was almost anticipated by Regner de Graaff as early as 1665), and by his recognizing the germ layers formed by the first divisions of the egg cell and out of which certain groups of organs develop, and finally by his justifiable warnings against the very general application of Meckel's parallelism, he succeeded in dominating the whole of morphological biology during the nineteenth century.

But von Baer was also conscious of the consequences of his embryological work, consequences which were of importance for distinct groups of problems such as those involved in the cell theory and in the doctrine of evolution. Thus his successors, amongst whom A. von Koelliker once again occupied a prominent place along with F. M. Balfour, Oscar Hertwig and many others, found the way prepared for the subsequent development of morphological embryology.

Morphological embryology was complemented by physiological embryology. At first the science was merely observational, but later it became experimental. Embryology penetrated further into the origin of the individual until it reached the fertilized egg. It liberated the processes of fertilization from the shroud of mystery which had hitherto enveloped them, and it established the equality of the female ovum with the male spermatozoon. Thus it demonstrated

Figure 46. Karl Ernst von Baer (1792–1876).

the futility of the prolonged battle between the spermatists and the ovists. The work in this field of Oscar Hertwig and E. van Beneden, *inter alia,* almost made a connection with the discoveries by E. Strasburger that added so much to our understanding of the fertilization of plants—discoveries that were made at the same time (1880). These researches probed more and more deeply into the mysterious structure of *cell* and *nucleus* (A. Weismann), researches which invested embryology with a broader biological importance.

From embryology, there sprouted an offshoot—experimental embryology—which combined once more with experimental anatomy—with the study of developmental processes—so well established by W. Roux, and so magnificently developed by H. Spemann. At the outset, Spemann concentrated his attention on the influence of the

external factors that affected the early development of the embryo, but later he extended his investigations into the internal factors—into the hereditary predispositions—thus touching on the domain of heredity, the domain of genetics.

This trend in embryology did not consider the organism—its complex of organs and tissues, and its developmental phases—as mere snapshots of vital phenomena, but as motion pictures which recorded the actual vital processes. This field is now labeled "physiological embryology." By its very nature, it is connected intimately with zoological physiology, discovered by Vesalius, Fabricius, and Harvey, and dominated until well into the nineteenth century by Haller. Here too, for the time being, only isolated facts were available. In spite of all Haller's efforts, a meaningful unity was lacking, a unity which might have been obtained by the comparison of physiological processes in different species of animals and by the linking together of different processes in the same species.

It remained for Johannes Müller to see that the same progress which was achieved by anatomy, when it altered its course to become comparative anatomy, could also be achieved by physiology. His *Handbuch der Physiologie* (1833) was the fruit of a rich experience and a careful evaluation of the available results. In this book, he made the first attempt to construct a comparative physiology and here he defended the rights of physiology to serve as the basis for psychology. He adopted this latter viewpoint early in his career—in fact, when he graduated in 1822. He expressed the conviction: "Psychologus nemo nisi physiologus"—"No one can practice psychology without simultaneously being a physiologist."

Johannes Müller had a complete command of comparative physiology in all its aspects, and he had the ability to employ all of its methods. This achievement was rare at the time and was later to become almost unattainable. After Müller, the comparative method was abandoned almost entirely, only to reappear at the very end of the century. A few scientists still more or less retained their grasp of the whole complex: Claude Bernard gave the classic example of this in his *Leçons sur les Phénomènes de la Vie Communs aux Animaux et aux Végétaux* (1878). For the rest, especially in animal physiology, a sharp and limiting antithesis manifested itself. This antithesis had appeared earlier in plant physiology; it was a divorce in method between a physical and a chemical technique of research.

Physical physiology, which attempted to measure processes and

record them with the aid of instruments, owed its original success to the ingenious notions of K. Ludwig, the founder of a graphic method (1847) that made it possible to register rhythmical changes of pressure on a revolving drum. A second source of progress it owed to E. du Bois Reymond, who created a fertile field of research by the application of electric currents to animal life (1848). Finally, the science became indebted to the concepts of H. V. Helmholtz, whose *Lehre von Tonempfindungen* (1862) and *Physiologische Optik* (1866) were physico-physiological masterpieces. These three men taught us to see as physical process such activities as the contractions of the heart and of the pulse, and to measure such factors as blood pressure, respiratory rhythm, muscular and nervous reactions to stimuli, the rate of transmission of nervous impulse, and the subtle action of the senses. They taught us to explain all such activities by physical hypotheses.

Chemical physiology actually originated in a discovery of F. Wöhler who in 1828 proved that it was possible to obtain organic urea from inorganic substances, and in the work of J. v. Liebig, who held that metabolism in animal organisms was due purely to chemical reactions. Hence, there grew up side by side with chemical physiology, a physiological chemistry, whereby the emphasis shifted from physiology itself to chemistry. This shift was aided by G. J. Mulder in the period from 1843 to 1850, when he published his standard work, *Outlines of General Physiological Chemistry.* In this *volteface* from chemical physiology to physiological chemistry, however, a growing danger lurked, because of so much help being given to this branch of physiology by pure chemists. Among the chemists who almost swamped the physiologists were A. von Baeyer and Emil Fischer, who synthesized all sorts of organic substances, including even some proteins, and Wilhelm Ostwald by his work in the physico-chemical aspects of fermentation. The great majority of physiologists, however, were still influenced by Johannes Müller. Müller managed again and again to incorporate the discoveries made in chemistry into genuine physiology, and in so doing continued to guard the biological aspects of physiological chemistry.

A deeper study of anatomy resulted in producing an offshoot, histology. From histology the study led into the primary constituent of living matter, the cell, which is the ultimate unit that can form an organism in itself, or can serve as the basic element out of which the more complicated plants and animals are constructed. Physi-

ology developed along lines parallel with anatomy. It passed from organic physiology, where the method of physics predominated, through the physiology of tissues, where the methods of investigations were more physico-chemical. Supported by such powerful speakers as M. Verworn (1863–1921), it developed into a physiology of cells. It even grew into a comparative cell physiology. Here it renewed contact with the sciences from which it had become estranged—the sciences of botany and microbiology. It is chemical physiology which, as biochemistry, now connects the related subjects of botanical physiology and microbe-physiology into a whole.

The same type of development—differentiation followed by a re-synthesis—as shown by zoology was followed by botany. Here the sequence was morphology—anatomy—histology, terminating in cell research, or *cytology*, and in cytology it reached the domain of general biology. Embryology, the science that traces the development of the organism back to the fertilization of the egg—where it links up with botany, microbiology, and physiology—joins biochemistry and thus also returns to general biology. This same type of development, the differentiation into morphology-anatomy-histology-embryology-physiology, also took place in botany.

Just before the beginning of the century, in 1795, Goethe had introduced the concept of "type" into zoology. Five years earlier he had written his *Versuch die Metamorphose der Pflanzen zu erklären*, a short work that established plant morphology as a science. He realized that the exterior regions in a plant, but not in an animal, were subject to individual influences that were not dependent on the internal structure, and that in the higher plants, at least, specific vital phenomena play a part that is confined to the surfaces.

Goethe supported the theory that in these higher plants, the seedlobes, the foliage leaves, the stamens, and the carpels were modifications of the same fundamental structure. He supported this hypothesis of metamorphoses by citing numerous observations. Goethe's theory gave the initial impulse toward the formation of formal or theoretical plant morphology.

Forty years passed before K. F. Schimper and A. Braun presented their colleagues with the theory of a mathematically determined phyllotaxis (1835), and another forty years passed before A. W. Eichler identified the elements composing the structure of flowers in his classical *Blütendiagramme* (1875–1878). Now at last, plant morphology had been given a complex of hypotheses that it could

test and on which it could build. In this manner it became an independent science, but a science that demanded of those who practiced it a "special morphological intuition." It expressly avoided all contact with other closely allied branches of botany—such, for example, as plant anatomy and the study of plant development. In fact it consciously withdrew into an isolated position, but it remained firmly convinced that its data and its results were invaluable and also that it had an especial importance to plant taxonomy. But its destiny lay in the hands of a very small group of zealous botanists.

In addition to its main line of growth and development, plant morphology could advance also along other lines. Like zoology it could become comparative and experimental; it could also investigate the forms of plants, either in their adult stage or during the course of their development. However, when these methods are applied to botany, they meet with far greater obstacles, because the organs of a plant are much more susceptible to passing and sometimes uncontrolled environmental influences. Yet here, too, the example of the zoologists was followed: the idea of "type" and the concepts of homology and analogy were introduced into comparative plant morphology. It was in this field that the work of Karl von Goebel was pre-eminent.

Experimental work on the higher plants was also pursued by H. Vöchting, among others, and on the lower plants also, where G. Klebs was the leading investigator. Payer's excellent study *Traité d'Organogénie Végétale Comparée de la Fleur* (1857), a worthy precursor to Eichler's *Blütendiagramme,* dealt with the development of the various parts of the flower. But these researches, owing to the nature of the subject matter, have not led to a completely satisfactory result in botany—in contrast to the results obtained in the corresponding sections of zoology. Comparative morphology and developmental processes of plants have not, as yet, attained the status of a fully developed and organized science.

On the other hand, since the time of Grew and Malpighi, plant anatomy has grown into a satisfactory and self-consistent whole. Its broad outlines had already been drawn by these two seventeenth-century precursors. The vast number of plant anatomists in the first half of the nineteenth century, especially those of German origin, could do little more than eliminate certain errors, fill up certain gaps and bring the developing field of knowledge nearer completion. The greatest obstacle they had to surmount proved to be the nature

of vegetable fibers and plant vessels. Finally, Treviranus (1806) and von Mohl (1845) recognized that these also were products derived from cells. Thus the cell acquired the key position of a structural unit in the plant body.

However, a profound divergence was discovered between the anatomy of plants and that of animals. In animals the anatomy of organs always remains separate from the anatomy of the tissues, because an animal organ may be and generally is composed of many different tissues. Thus in animals the organs and their descriptions —their *organography*—present a field of research that is distinct from the investigations of tissues per se. This investigation of tissues we have listed earlier as the science of *histology*. Moreover, a further complication arose in the fact that in animals both organs and tissues are composed of cells—often of many different kinds of cells. The intensive investigation of the structures and activities of cells constitutes a separate and specialized field of its own, a field we call the science of *cytology*.

Thus, because of the complexity of the animal organs, their study involved research in three distinct sciences: anatomy, histology, and cytology. On the other hand, in the botanical field, these sciences overlap. In fact, an anatomical analysis in plants leads directly to the cell—to cytology—so there is no real and separate science of plant histology. Incidentally, as both plants and animals are composed of cells, cytology operates in a field where botany and zoology are intimately joined. Thus it is a part of the over-all science of biology.

Thus, because of the way plants are constructed, the best plant anatomists of the first half of the nineteenth century—C. F. Mirbel, J. J. P. Moldenhawer, H. F. Link, C. L. Treviranus, F. J. F. Meyen, and, especially, Hugo von Mohl and Carl Nägeli—paid little attention to plant tissues as such. Instead they concentrated their attention on the cell as the structural element in all vegetable organs.

After 1850, a real comparative anatomy of plants began to develop: de Bary's *Vergleichende Anatomie der Phanerogamen und Farne* (1877) remains the great classic on this subject. A fundamental concept in plant anatomy—the stela theory—was elaborated through the labors of Ph. van Tieghem (1870) and J. C. Schoute (1902). The stela theory explains the architectural principles of the columnar structure of the higher plants. The different cells which go to make up the still homogeneous tissue of the growing-tip

were described in outline by Hanstein (1868). Schwendener (1874) on his part examined and described accurately one of its important components, the supporting cells.

A new and entirely virgin field for study was that of plant embryology. The first prerequisite for its development was the irrevocable definition of the sexual process in plants. The work of Camerarius, Koelreuter, and Sprengel had demonstrated this for the higher plants. Their researches had clearly defined the role of pollen as the fertilizing substance and, in his *Metamorphosis* (1790), Goethe affirmed that he was completely satisfied that the theory was correct.

Thirty years later, however, he took a diametrically opposed position in a short treatise, *Verstäubung, Verdunstung, Vertropfung* (1820). Henceforth he held the opinion that the function of the stamens was nothing but a "pulverization" of harmful substances, an action on the part of the plant to rid itself "of the oppressive dust, so that the whole of the innermost essence may finally gush forth with vital force to reproduce ad infinitum."

Goethe was supported in this strange aberration by the publications of some "official" botanists (F. J. Schelver, 1812, *Kritik der Lehre von den Geschlechten der Pflanzen*, and A. W. E. T. Henschel, 1820, *Von der Sexualität der Pflanzen*) and his general and widespread influence undermined the foundation of the sexual theory of plant reproduction in the minds of his admiring public. That is, until the independent spirit of C. L. Treviranus, in 1822, finally put an end to such extraordinary notions with his *Die Lehre vom Geschlechte der Pflanzen.*

Treviranus had stated unambiguously what the principles for further investigation were and how they should be pursued. Future research should be carried out, he stated, along three different lines: (1) It should be on the biology of flowers, which had already been sketched in outline by Sprengel and was still waiting further expansion, a task that was begun by H. Müller in 1873. (2) The actual process of fertilization, the question as to what happens after the pollen has landed on the stigma, should be investigated. This was the fallow soil on which embryology was waiting to grow. And finally, (3) there was the problem of hybridization, which was to prove to be the same in both plants and animals.

The early development of the young plant embryo—its growth from the fertilized egg—takes place deep down inside the ovary and therefore is not easy to examine. The prevailing opinion of the time

was that the pollen grain bursts and its contents diffuse down through the style. Amici proved, in 1823, that this was incorrect; he demonstrated that the pollen grain germinates, forms a tube, and transfers its content through the tube into the ovary, where the actual seed contains a translucent structure which is "stimulated" to further development by the pollen tube.

Twenty years after Amici, Wilhelm Hofmeister dominated plant embryology through his masterly microscopic technique. In 1849 he gave a description of the formation of the phanerogram embryo, a description which was so nearly perfect that the most rabid antagonists of Amici had to yield and accept it. It was Hofmeister who really founded plant embryology. But at this time, doubts began to arise in botanical circles as to the correctness of the dogma that the lower plants—the ferns, mosses, fungi, and algae—were devoid of all sexuality. Some suspicions had already arisen in the time of Koelreuter and Linnaeus. The opinion was expressed even then that these organisms possessed sexuality like all the other plants. But this suspicion did not become an established fact until W. Hofmeister in 1851, F. Unger in 1854, and N. Pringhseim in 1856 published their pioneer investigations, which established once and for all the sexual reproduction of the cryptogams. In this case, however, it was not possible to speak of a real "embryology," since the process in the cryptogams was restricted to fertilization itself.

But the sexual theory of reproduction in plants gave rise to an important premise that made it possible to connect the reproductive process in plants with the reproductive process in animals. The essence of sexuality lies in the fusion of two cells into one. The significant question as to what the universal process of fertilization might be was thus given a preliminary answer. Further investigation of the phenomenon could be left to general biology.

Finally, there was the field of plant physiology. Here the way had been prepared by Hales, Ingenhousz, Senebier, and de Saussure. These early physiologists had already pointed out the two possible trends in the science, the physical and the chemical. Zoology had made grateful use of this hint. In botany, these trends were accepted also as "Leitmotives," but in botany the basis of further specialization was not the methodology so much as it was the vital processes themselves. The metabolism, through which the plant obtains its nourishment from carbon dioxide and inorganic substances and through which these constituents are further assimilated

and broken down, is a whole science in itself. The transportation of these substances within the plant, which is made possible by the absorption and evaporation of water, comprises still another section of plant physiology. The growth and movement of plants also constitute another more or less independent discipline. In each of these fields, innumerable scientists of greater or lesser individual importance were active in their various investigations. The whole of plant physiology, however, was dominated by two personalities: in the first half of the nineteenth century by Sir Andrew Knight, and in the second half by Julius von Sachs.

The absorption of mineral substances was the special subject of J. v. Liebig; the assimilation of nitrogen was first examined by J. B. Boussingault. The photosynthesis of carbon dioxide into starch had been described earlier by Ingenhousz but by the nineteenth century it had grown into almost a science in its own right. In the field of the absorption of water and salts, we have the osmotic studies of W. Pfeffer and Hugo de Vries, both of whom published in 1877. In the field of evaporation and water transportation Sachs again was foremost but there were many others who also labored effectively. In addition to these well-defined and separate divisions of plant physiology, physical processes were uncovered where no chemical reaction took place, as in the evaporation and the absorption of water. There were also chemical reactions without a physical background, as in the assimilation of carbon dioxide and in certain other processes of metabolism. Combinations of physical and chemical processes were also found in the circulation of matter, and especially in the problems that have recently acquired such great importance through the contributions made by the school of F. A. F. C. Went—problems of growth and of phytohormones.

Where plant physiology fused with chemistry, its findings were relevant not only to the higher plants but also to the lower—even to the unicellular plants. Here botany joined microbiology. Through microbiology it again arrived at the cell. In the reactions that occur within the cell—such as that in the actions of enzymes, in the release of energy, and in the production of metabolites—zoology, botany, and microbiology exchanged information and supplemented each other. Here, physiology almost fused with biochemistry.

Biology takes as its province not only the normal life of plants and animals but also the abnormal. It is concerned with malformation and disease. These subjects also have their places within the

complex of biological subdivisions. In animal pathology, however, greater store was set on its independence and, in the hands of veterinary surgeons, it attempted to become an independent profession like medicine. It seemed reluctant to be regarded as a part of biology.

Plant diseases, on the other hand, were more at home in the biological milieu and fit readily into the section of *phytopathology* that has been assigned to them. *Phytopathology* derived its data for the diagnosis of diseases from anatomy, its data for determining disorders in the constitution of the plant from physiology, and its technique for tracking down the culprits in parasitic disease from microbiology, mycology, or entomology. It obtained useful knowledge from the pathology of man and beast and, in cooperating with soil analysis, it traced the detrimental consequences of the absence of a few important minerals—the trace elements—which are normally present only in extremely small quantities.

Phytopathology owed its origin in part to the studies on fungi that M. J. Berkeley made around 1850, and to the later discoveries of H. A. de Bary and M. Woronin. It was also indebted to the agricultural work of Julius Kühn and to a number of others. It is still somewhat lacking in unity, but it is growing and flourishing and, in a comparatively short period, it has managed to attract the attention of large numbers of very able biologists.

Every biological investigation must begin by identifying the specimen to be examined and ascertaining where it fits into the overall system of living things. Thus biology, as an organized science, always begins with the classification of all the known forms of life —of unicellular organisms, of plants and of animals. But this classification—this systematization—has not only the duty of speaking the first word, it has also the right to speak the last. Each component part of botany and zoology sheds light upon only a single facet of a single organism or of a limited group of organisms. Systematics has the task of surveying the whole of nature and, in so doing, to unite in one whole all the many facets revealed by the numerous techniques of all specialized biological research.

The main directives given by Linnaeus in his *Systema Naturae* were applied chiefly to botanical material. His first large-scale attempt at devising a system of plant classification soon gained in depth and refinement, especially through the efforts of the Frenchmen A. L. de Jussieu and A. P. de Candolle. Plant classification

after Linnaeus ran a relatively smooth course; it was spared world-shaking changes, and it gradually filled in the gaps, particularly those caused by an incomplete knowledge of the lower plants, i.e., the cryptogams.

Thus far in our account we have mentioned only the nineteenth-century contributions to biology made by the scientifically advanced Europeans. These active and productive biologists, moreover, were Europeans who remained in their home continent. Those transplanted to or born in America lived too far from the rapidly developing intellectual centers, either to keep up with the newer discoveries or to add their quotas to the ever growing store of specialized information.

The Americans, however, had one advantage. They lived in a continent with a flora and fauna of its own, a continent where new species could be found almost anywhere, and where the problems posed by the distribution of species were too obvious to be missed. In a period when systematics supplied the only real unifying force in biology and when its importance was universally recognized, the American biologists naturally took advantage of their many undescribed species, and a number of them developed easily and painlessly into systematists. Their chief contributions were in plant systematics and in the systematics of plant and animal fossils.

The few competent American botanists soon found themselves swamped with specimens. The frontier of the United States moved westward in great jumps. With each advance the national government sent out numerous well-equipped exploring expeditions in order to take an inventory of its new acquisitions. Most exploring parties included a plant collector, and the plants he collected were shipped east and stored in museums and laboratories. They had to be worked over and identified by the botanists. Thus the American systematists had not only their own collections to arrange and classify but also collections made from one to three thousand miles from their places of residence.

A history of plant systematics would include the names and contributions of these Americans. Here we can merely state that important additions to the taxonomy of the higher plants were made by Amos Eaton (1776–1842), John Torrey (1796–1873), George Engelmann (1809–1884), and Asa Gray (1810–1888), and to the taxonomy of the cryptogams by W. S. Sullivant (1803–1873). Leo

Lesguereux (1806–1883) investigated the paleobotany of the American coal beds and did much to advance fossil botany.

In addition to his contributions to systematics, Asa Gray investigated plant distribution and soon became one of the founders of plant geography. He did more perhaps than any other botanist to discover and recognize the significance of the strange crisscross distribution of plants in the Northern Hemisphere. The European plants are closely related to those of the Pacific coast of North America, while the nearest relatives of the plants of eastern North America are found in China. This distribution was completely unexpected. Its explanation, when it was finally discovered, fitted well into the Darwinian theory of evolution. Indeed, Asa Gray was one of Darwin's confidants and, when Darwin and Wallace presented their joint communication to the Linnaean Society in 1858, Darwin included in his presentation a letter he had written to Gray in the previous year.

The destiny of animal classification was entirely different from that of plant classification. The course of its development did not run smooth, and it did experience a world-shaking change. But here, too, the main directives given by the father of classification, Linnaeus, remained intact. Soon, however, the growth of comparative anatomy made its influence felt. This changed the basic scaffolding on which animal systematics rested. Ever since the time of Linnaeus, the opinion had been firmly rooted, that all animals—from the lowest to the highest—formed one continuous series. Therefore they were classified along a single line, a *scala naturae*.

In his *Système des Animaux sans Vertèbres* (1801), Lamarck had at first followed this method of classification, but some years later, in his *Philosophie Zoologique* (1809) he deviated from this system on a number of important points. Instead of ranging his animal groups along a straight line, he arranged them along branching lines. "It may be seen, that in my opinion the animal scale begins with at least two separate branches and that, along its course several ramifications seem to bring it to an end in specific places." (*Philos. Zool.*, II, ed. 1809, p. 462; ed. 1873, pp. 423–424; cf. Figure 55, p. 308.)

This concept introduced a whole new train of thought into systematics. It established the fact that the relationship between separate groups of animals—a relationship recognized by Lamarck himself—could be due to their common origin. This connected the idea

of evolution to that of systematics. Soon it became evident that it would be possible to build up another system side by side with that of a classification according to structure, i.e., a classification according to descent—one based on relationships. Thus *phylogeny* made its appearance. Phylogeny found its more consistent treatment in the *Botanische Stammesgeschichte* that J. P. Lotsy published a hundred years later.

Cuvier, for his part, established the connection between the classification of animals and comparative anatomy. The four "types" drawn by him on the lines of comparative anatomy passed into the system of classification: his book, *Le Regne Animal Distribué d'après son Organisation* (1816), was the result of the concept of "type" as it was worked out in practice. Here too, *Distribué d'après son Organisation* meant a new element in systematics.

Next to Cuvier with his comparative anatomy came von Baer, with the embryology of animals as an additional criterion for the system of classification; i.e., the development of the animal was an indication of the place it should occupy in the system. This principle, of which both Cuvier and von Baer were protagonists, was to be of great value. The lines laid down by Cuvier were later to be revised and elaborated by K. Th. von Siebold and R. Leuckart, and the idea of "affinity" was to be amplified. Here, in spite of their difference in outlook, Lamarck and Cuvier produced something between them that was of real value.

The new trend in zoological classification culminated in a treatise that would later become famous, the *Essay on Classification*, published by Louis Agassiz in 1859—the year in which Darwin's *Origin of Species* was also to see the light. Agassiz had a broad vision; he understood that a taxonomic system was not to be built upon morphological-anatomical data alone, and that von Baer's embryology, a portion of physiology that emphasized the distribution of living plants and animals, and especially paleontology, were all subjects which could aid in establishing sound principles of classification. As a systematist, Agassiz left little to be desired, but we remember him today because of his marked intellectual limitation. He could never accept Darwin's explanation of how species could have come into existence.

The petrified remains of plants and animals, the fossils, had been known even to the ancient Greeks. Fossils had always stimulated the human imagination. In attempting to explain fossils, the biolo-

gists found themselves face to face with enigmas whose solution was neither simple nor obvious. Fossils left ample room for the imagination and gave rise to mystical speculations as to the purpose for which the Creator had intended them. They were often regarded as freaks of nature (*lusus naturae*), models designed by the Creator in attempts that were not quite successful and whose creation had been abandoned because of their flaws and imperfections. Gradually, the notion gained ground that fossils were the remains of animals and plants which once had lived. N. Steno, in 1669, furnished definite proof in support of this belief.

But the question arose: how and through what cause had these organisms disappeared from the face of the earth? This too was explained: the Flood had destroyed them. At first this seemed fairly plausible, but obstacles were not slow in appearing. As the fossil material accumulated, types became more and more varied. The presence of fossils of large animals in high mountainous regions and of smaller, lighter forms in the plains seemed inexplicable. The enormous differences in the depths at which fossils were found and the superimposed layers, each with its own typical fossils, rendered the Flood hypothesis even more improbable.

The year 1796 brought the turning-point: Cuvier communicated to the Paris Institut his first information about the fossilized remains of elephants found in the vicinity of Paris. For twenty-five years he pursued these researches with all his energy. His monumental work, *Ossemens Fossiles* (1812), became the basis of a new science—the *paleontology* of vertebrates. Evidence accrued showing that whole series of animal species had become extinct one after another, and that new species and even new orders had appeared repeatedly in the more recent strata. The Flood hypothesis became absolutely untenable. Cuvier himself found a different solution. He held that at intervals the earth had suffered a series of immense catastrophes and that the Flood was only the most recent catastrophe. During each catastrophe, he held, a large number of the existing species were destroyed, and the earth was then repopulated with newly-created animals.

Around 1810, Cuvier's great contemporary, Lamarck, and simultaneously William Smith, supplemented his work with their investigations of the layers in which the fossils of the invertebrates were found. Invertebrate fossils were also discovered in large quantities. The observations of Lamarck and Smith on the invertebrates ran

parallel to those of Cuvier on vertebrates, but Lamarck interpreted his fossils quite differently. To him they served to strengthen his theory of evolution. Cuvier and Lamarck looked upon their fossils primarily as zoological objects. On the other hand, Smith was a mining expert who combined his great experience in this field with accurate observations on the strata in which the different fossils were found. Accordingly, he was able to point to the fact that these organic remains appeared in layers, that certain forms of fossils were typical of specific formations, and that there was order and a definite relationship between the successive strata and the fossils which were found in them.

The work of Cuvier, Lamarck, and Smith built a valuable scaffolding for the paleontology of animals, a scaffolding on which C. Lyell, R. Owen, L. Agassiz, T. H. Huxley, E. D. Cope, O. C. Marsh, and K. von Zittel could build with safety. Thus, a groundwork was laid which was to be a solid and indestructible foundation, both for the classification of animals and for the evolutionary history of life. It would also serve as the basis for the science of historical geology.

It was in the field of paleontology that the American biologists were able once again to make a major and unique contribution. This was due not so much to their talents as it was to their location. As the western plains of the United States were explored, enormous and rich fields of fossils were discovered. It later turned out that the horse and the camel—both Old World animals—had evolved in this region and that, much earlier, many of the giant reptiles had also evolved there. Whole petrified forests were found, and interest in fossils was widespread and intense. The leading American paleontologists were Joseph Leidy (1823–1891), E. D. Cope (1840–1897), and O. C. Marsh (1831–1899). Both Cope and Marsh were men of independent means. They were bitter rivals, and the feud that developed between them has become a classic.

However, these active, able, and egocentric men advanced greatly the science of paleontology. Marsh, for example, described the flying reptile *Pterandon*, and he and his assistants found and described the *Brontosaurus*, the *Allosaurus*, and the horned *Triceratops*. He also described some thirty odd species of the *Equidae*. His descriptions assisted greatly in tracing the ancestry and the course of evolution of the horse.

It was not long before some fossils were found that were involved directly in the evolutionary history of man. These were the fossil

remains of man-like creatures, of which *Pithecanthropus,* discovered by E. Dubois, was the best-known. These fossils now constitute one of the most important elements of *anthropology,* the science that had been founded by Blumenbach.

Meanwhile the botanists had also gained access to fossilized plant remains. Eighteenth-century investigators, of whom J. J. Scheuchzer with his *Herbarium Diluvianum* (1709) was the first, had already printed illustrations of fossil plants, but the books they wrote cannot be said to have been of great scientific importance. The development of *paleobotany* had to await the contributions of the French school that arose early in the nineteenth century, when A. Brongniart initiated the scientific treatment of fossilized plants with his *Histoire des Végétaux Fossiles* (1828–1835). This was soon followed by the *Fossil Flora of Great Britain* by J. Lindley and W. Hutton (1831–1837).

Following the publication of these works there was a rapid increase in the number of paleobotanists. H. R. Göppert, W. Ph. Schimper, and A. Schenk have each made valuable contributions to the continuing development of paleobotany. The study of plant fossils has become of vital importance to botanical systematics, which aims at a survey of the entire vegetable kingdom in all of its aspects and relationships. This study filled in many of the gaps among the living plants and it has brought to light a multitude of the transitional forms.

The fossil flora and fauna give us a picture of the distribution of species as they were disseminated over the earth in previous eras. Their present remnants in different parts of the world are an important aspect of plant and animal geography. The field of the biogeographers covers the entire earth; each region has its own flora and fauna, and plant geography attempts to view the flora of the world as a whole.

After the start that was made in this field by Alexander von Humboldt and A. Bonpland in their classical *Essai sur la Géographie des Plantes* (1805), plant geography was developed further by A. de Candolle in his *Géographie Botanique Raisonnée* (1855) and by A. F. W. Schimper. The world of plants was now divided into vegetation zones, which we list as the Arctic, Western Europe, Central and East Europe, the Mediterranean countries, Southern Russia and Hungary, North Africa, Eastern Asia, North America, South America, tropical Africa, the Cape of Good Hope region, and Australia.

Each of these regions constitutes a more or less independent vegetation zone. All of them have been and are still being investigated intensively. The information obtained from these studies constitute the new field of *plant geography*.

Of great importance for this type of study, are the botanic gardens in the separate zones—e.g., in the tropics, that in the Buitenzorg (Progor) Garden on the Island of Java, which became, under the auspices of Melchior Treub, a truly universal center of research. But the evolutionary changes in the plant geography of a region can also be the subject of investigation. These changes in the flora of a given region, which have taken place in the past, and which are still occurring, form a separate branch of study, a branch which A. de Candolle named "epiontology."

On the other hand, plant geography may confine itself to a small, accurately-defined surface. The encrusting lichens growing on the rock of a particular geological formation are just as much a "flora" as the plants in a meadow or as those growing on a moor. The flora in a square yard of forest is quite different from that in a neighboring fen. This restricted aspect of plant geography is known as *plant sociology*, the study of the relations binding a species of plant to its environment. Zones of vegetation and epiontology can only be treated by descriptive and comparative methods. Plant sociology, however, has an intimate bearing on physiological processes. Consequently it has to enlist the help of experimental research in solving the problems that confront it.

Almost parallel to this system of plant geography runs the study of the over-all distribution of animal species, that is, *zoogeography*. Early in the nineteenth century, this too was given a start by Alexander von Humboldt when he edited the material collected in his travels. General zoogeography includes the fauna of land, sea, and atmosphere. Analogous to plant geography, these faunas are subdivided into a Notogaea (Australia), a Neogaea (South and Central America), an Arctogaea (the rest of the world between the polar spheres), and an Arctic and an Antarctic (the two poles). Each of these spheres is again split up into smaller dispersal zones. Historical zoogeography dates back to the work of Charles Darwin and A. R. Wallace; it seeks the origins of the various faunas. Finally, ecological zoogeography runs parallel to plant sociology: the animal in its environment constitutes the basis for the study of the relation of the species to its specific surroundings.

Thus, in the nineteenth century, the scattered and relatively incoherent studies of the eighteenth-century biologists and their predecessors grew into an organized whole. The increase in the compass and depth of the research material made it necessary to divide it into many different sections, each focused on a specific aspect of life. At times this differentiation and specialization threatened to disturb the harmony and the unity of biology. But, in innumerable cases, a mutual collaboration between the individual sections of biology was found to be essential if they were not to bog down in some highly specialized blind alley. Microbes, plants, and animals each confront us with their own specific microbiological, botanical, or zoological problems, but they also show us many attributes of a more general biological nature. They make it possible for us, by concentrating on our common ground, to draw nearer to the goal of all biological research, which is the study of life itself.

11

Concentration

In the preceding chapter we have listed some of the more important discoveries that were made during the course of the nineteenth century. They were made in such numbers and in such diverse fields that no biologist could possibly verify them all—certainly not if he wished to do any creative work himself. Thus the biologists were compelled to concentrate their interests and their investigations. Often they had to limit their activities to one small field, but they still had time to maintain a background knowledge of the sciences as a whole. They never had to lose the logical connections between the different fields of research and, to them, the basic unity of biology was never obscured. And this was of major importance for progress in the science because, every now and then, the information that had accumulated in the different specialties would fit together into some general pattern and, when this occurred, biology would make one of its great theoretical advances. Perhaps the greatest and most revolutionary of these—certainly the most spectacular—took place toward the middle of the century, when the doctrine of *organic evolution* was put on a sound factual and theoretical basis.

But all of the broad generalizations that arose during the century were not so propitious. Some, especially those that had philosophical implications, turned out to be little more than a spilling over into biology of one or another of the fashionable attitudes toward life in general. Many such attitudes were popular—attitudes that were conditioned by the more popular ideological stances of the

day. One of these, perhaps an offshoot of the Romantic reaction against eighteenth-century empiricism, was known as *Naturphilosophie*. This doctrine became a major factor in the orientation of many German and Scandinavian biologists. The biologists in England and France, however, escaped its full impact and, as time passed and the science of biology continued to acquire reliable information, *Naturphilosophie* faded slowly away. But, during its heyday, it exercised a major influence which spread well beyond the science itself.

Here we can give *Naturphilosophie* but little space. The doctrine seems to have come into being through an interaction of the romantic notions of Goethe with the philosophical ideas of Immanuel Kant (1724–1804) and Friedrich Schelling (1775–1854). Its most active advocate among professional biologists was the versatile but eccentric Lorenz Oken (1779–1851), né Ockenfuss. In spite of the fact that Oken was tactless, rather violent in his political convictions, and continually getting into trouble, he was, for a while, exceptionally influential. He organized meetings where scientists could get together and discuss their problems, and he founded a magazine, *Isis*, which published papers on various controversial questions. Perhaps the major accomplishment of his life was the awakening of a popular interest in nature.

Oken was forced to leave the faculty of the University of Jena in 1819, and the faculty of Munich in 1832. He ended his days at the University of Zurich. He obtained most of his general ideas apparently from contemplating his own unaided, inner consciousness, and we can dismiss his heavier thoughts as so much gibberish. However, as early as 1805, he published a very influential book, *Die Zeugung*. In this book he indicated how important the vesicles or cells were in the origin and structure of all organisms. This, at least, is to his credit.

Another Naturphilosoph, worth perhaps a passing mention, was Christian Gottfried Daniel Nees von Esenbeck (1776–1858), who studied medicine at Jena, where he fell under the influence of Goethe and Schelling. Esenbeck was a man of independent means who lived on his private estate until 1818, when he was appointed professor of botany at Bonn. At Bonn he established a botanic garden. Later he accepted a chair at Breslau. He did some excellent systematic work in classifying the plants sent him from Brazil and from Africa, and his work on these tropical floras is still

respected. However, it is an act of pure kindness on our part not to quote his more general biological pronouncements. In his old age he became ultra-radical. He joined the labor movement and he set out to reform Christianity and to abolish marriage. He did not succeed in either of these enterprises.

We shall conclude our treatment of this phase of biology by citing very briefly the work of a third Naturphilosoph, Carl Gustav Carus (1789–1869). Carus was professor of comparative anatomy at Leipzig and later professor of gynecology at Dresden. He was a man of varied interests and a personal friend of Goethe. He wrote on painting and was himself a painter. He also wrote on comparative anatomy. His *Natur und Idee,* published in 1861, contains many elements of mysticism and more than a little evidence of confused thinking. He pictured the universe as an infinite sphere whose central point was everywhere and whose periphery was purely ideal. He may even have been reaching toward some sort of a theory of relativity. His comparative anatomy of the central nervous system, however, was much more specific and very well done.

As we have noted, the growing need of specialization threatened to fragment biology into a number of disparate disciplines and to break it up into fields that would lose in breadth what they had gained in depth. This threat, of course, is still with us. It is now, perhaps, more menacing than ever. But, in the nineteenth century, the threat was met and checked by a number of broad and general discoveries, whose theoretical implications tended to unite the separate specialties into a single unified science. First, there was the emergence of the cell theory, a concept that applied to all living things. Then too, the discovery of the nucleus and of the universal role of the chromosomes helped to bring together botany and zoology—the two great kingdoms of the organic world. The varied problems of heredity and the basic role of sexual reproduction were also found to be alike in the two kingdoms. A number of physiological principles, such as the role of oxidation in the release of energy, also had universal implications. Later on in the century, the discovery of the continuity of the germ plasm and of its plant analogue, the meristem, showed that all the multicellular organisms had so much in common that they were but variations on a single theme. Most striking of all was the theory of evolution, which literally united all the varied forms of life into a single family and

contributed to an understanding of all the separate biological specialties.

But over and beyond these unifying principles, there were factors within the specialties themselves which tied the whole mass of biological learning together. Neighboring specialties overlapped, and even distant specialties had to establish some lines of communication —lines that we can liken to telephonic connections between exchange centers, each center being linked with all the others to form a single and all-inclusive network, the whole forming what we know as "general biology."

Microbiology was concerned with the question of spontaneous generation, a question that obviously applied to all living things. Histology and anatomy in both botanical and the zoological fields met whenever the research in either subject reached the cell and, on the cellular level, they combined in the cytological investigations of the nucleus and cytoplasm. Physiological experiments on both animals and plants impinged on the vital activity of the elemental unit of all life, the cell. Thus, animal and plant physiology both contributed to cell physiology. The growing understanding of sexual reproduction in animals and plants led to the recognition that they had a common background and behavior pattern in the process of fertilization. An intensive investigation of this process revived the much older problem of "preformation versus epigenesis" and led ultimately to an intensive research into the principles of heredity.

The various nineteenth-century doctrines of heredity—now superseded by the science of *genetics*—were the product of a collaboration between the botanists and the zoologists. The rapprochement between the classification of plants and that of animals led to a general discussion of the concept of species as an over-all biological problem. It also raised numerous questions concerning the supposed immutability of species and their origin. The systematists together with the paleontologists endeavored to solve the problem of evolution, a collaboration in which geneticists were later to participate.

The dogma of spontaneous generation—of *abiogenesis*—that is, the doctrine that living organisms originate from non-living substances, had already been attacked by Anaxagoras, but now it seemed to have been doomed for good as a result of the experimental work of Redi and Spallanzani. Actually, these experiments *were* conclusive in the framework of the biology of the time. But doubt was not to be quelled entirely; there were some natural scientists who still cher-

ished the hope that they might one day save this dogma from the adverse pronouncements of biological experimentation. Objections were raised against the validity of the experiments; there seemed to be so few "germs" present in the air that they could not possibly have populated Spallanzani's test-tubes so prolifically in such a short time. Moreover, the air in his bottles was "tainted"—in modern terms "unoxygenated"—so how could negative results obtained by such methods have any real validity?

Thus the balance weighing the pros and the contras of spontaneous generation rested with its indicator at zero. It had appeared earlier that Spallanzani's experiments had upset the balance on the side of the "cons," but the balance regained its equilibrium. Because of this, the French Académie des Sciences in January, 1860 offered a prize for contributions to the following project: "Attempt, by means of well-devised experiments, to throw new light on the question of spontaneous generation." The committee appointed for this purpose set very high and rigid standards because the members were fully aware of the difficulties of any penetrating investigation: "The Committee requires that the experiments be accurate, exacting, examined in all their aspects and, to sum up, be such that the result may be one entirely free from confusion and be, moreover, the direct outcome of these experiments."

It was the great Louis Pasteur (Figure 47) who won the prize with his *Mémoire sur les corpuscules organisés qui existent dans l'Atmosphère. Examen des doctrines des générations spontanées* (1862). This truly classic paper proved conclusively the untenability of the concept of abiogenesis, at any rate insofar as it was concerned with the generation of living cellular organisms from nonliving substance.

Thus from the postulate accepted by the Greeks, that frogs and other vertebrates had their origin in the slime of pools or in seawater, we came to the notion that insects were generated spontaneously from rotting material, a notion that in Redi's time was still generally accepted. Subsequently, spontaneous generation was limited to the microbes, the "tiny animals" of Leeuwenhoek and Spallanzani, and ultimately to the "germs" of Pasteur's day, when the battle over the origin of all such organisms was decided.

As time passed, however, biology intensified its search; it tried to discover the agent of some age-old pathological symptoms—e.g., the "refraction" of tulip-colors, the infectious chloroses of *Abutilon*,

Figure 47. Louis Pasteur (1822–1895).

the degenerative diseases of potatoes, and the tobacco mosaic disease. In fact, it sought the agent in the infectious diseases for which no pathogenic organism could be held responsible, such diseases as smallpox, measles, mumps, jaundice, and infantile paralysis in man and foot-and-mouth disease, swine-fever, and rabies in animals. We can speak in general of such matter as infectious—in scientific language, Beyerinck called it a *contagium vivum fluidum*—but the French had already used the word *virus*, which has since come into general use.

Conclusive evidence of the actual existence of such infectious matter dates only from 1892. We do not know what a virus really

is, although we know that it is not a cellular organism. It can pass through a fine bacteria-filter. The largest variety (smallpox) attains the same dimensions as those of "ultramicrobes," of one eight-thousandth of a millimeter; the smallest are no larger than one hundred-thousandth of a millimeter and are therefore only slightly larger than a molecule of haemoglobin. The virus particle bears close resemblance to the colossal molecules of proteins, with molecular weights varying from some ten-thousands to several millions. They can be transmitted by insects and by such parasitic plants as dodder (*Cuscuta*). They may sometimes lie concealed in apparently healthy plants of one strain and only show their pathogenic characteristics when they are transmitted to another strain.

A few types of virus can be crystallized and preserved in a test-tube. All this is very important in itself but it shies away from the vexed point over which modern virus research is still at issue: is a virus alive or not? Some hold that it is, because the virus possesses a kind of metabolism and is capable of reproducing; others deny it life because one virus, the tobacco mosaic virus, can be crystallized and because these "crystals" do not grow or show signs of metabolism when stored in test-tubes. But against this it may be urged that bacteria too can live in the form of spores, in which case neither metabolism nor growth seems to take place, and the regular arrangement of particles, which is considered typical of crystals, can similarly be observed in various parts of plants and animals (fibers, muscle cells, spermatozoa).

Thus, virus research has landed us in "life's nebulous regions" as Kluyver recently and so aptly described it. Doubtless a considerable time will pass before definite proof is produced that such virus-matter, granting for the moment that it is alive, cannot originate by *generatio spontanea*, although up to the date of writing, there are no indications in this positive direction. Thus, the possibility of life arising from non-life has been reopened for those who are anxious to uphold spontaneous generation, but with this radical limitation: it must be admitted that no cellular organism can generate from non-living matter, but that possibly living matter that is not so organized may do so. The problem has become more and more remote, but it remains a problem. (*See* pp. 334 ff.)

In the past few years, great progress has been made in virus research. The resemblances of virus particles to Mendelian genes is, in some respects, striking. Both particles and genes are of about the

same order of magnitude and both can reproduce themselves only within living cells. The idea has recently been broached that virus particles may only be genes that have broken away from their chromosome anchorage—genes that have set out to lead an independent life. Indeed, heredity characters have been transmitted from one strain of bacteria to another by means of a *bacteriophage,* a virus particle of a definite and characteristic shape. Virus particles have now been photographed under the electron microscope. (*See* pp. 336 ff.)

Virus particles have also been broken down chemically into their protein and deoxyribosenucleic acid (DNA) components, and when they have been re-assembled they have regained their "life." The protein from one virus has even been combined with the RNA from another, and the synthesized "hybrid" virus has been as "alive" as a virus that occurs in nature (Fraenkel-Conrat and Williams). The protein seems to be necessary for the stability of the virus, but the specific DNA molecule seems to determine its infectious properties. As we know, genes also contain DNA, and in the possession of this component in common we can recognize an additional link between viruses and genes.

Recently the question of spontaneous generation has also been attracting the interest of the chemists. Amino acids, the building blocks of the proteins, have been made artificially by means of an electric discharge in a "reducing" atmosphere of ammonia, hydrogen, ethane, methane, and water (*Science* 117:528, 1953). But at this level, the problem of the origin of life must be investigated not only in the light of what the chemists have done but also in the light of the data obtained by the astronomers—data as to the past condition of our planet. It might also be well at this point to recall a point that had often been overlooked. We can be fairly safe in assuming that life is not still being generated spontaneously on the earth. Long before a carbon compound would become complex enough to become alive, it would become food. And hungry predatory organisms are everywhere.

But before life appeared on earth, as Darwin himself pointed out, the earth was sterile. Complex carbon compounds could have persisted indefinitely. Perhaps in some pool on the south side of a volcano, close to where local conditions of heat and pressure could have reduced the carbon to hydrocarbons, where phosphates, sulphides and the ammonium produced by lightning could be concen-

trated by evaporation, and where the potassium salts predominated over those of sodium, some such complex molecule could have been formed. For life to originate it would only be necessary for a single molecule of such a compound to become an autocatalyst, because such a molecule could impose its own structure on other molecules. It could grow and reproduce. We can if we wish call this kind of molecule alive. Perhaps some such molecule was our own ancestor. If it was, it gives us a very ancient lineage indeed.

In the early nineteenth century, however, the problem of the origin of life was considered to be on a much more primitive level. In fact, the primary structural unit of all animal and plant bodies—the cell—was still not recognized as such. Even after the cell was recognized as the structural unit, its own origin remained unknown. The anatomical analysis of the plant had led to a concept first stated clearly by M. J. Schleiden (1838): "But every more complex higher plant is an aggregate of complctely individual self-contained units, namely the cells themselves."

In order, however, to found a general biological cell theory, supplementary data were needed from zoology. It was only around 1830 that here, too, the light dawned. It is far more difficult to recognize the animal cell as a unit, because it has no distinct cell wall, yet the observation that the structure of the most divergent animal tissues consist of "granules" led Purkinjé in 1837 to link up the plant cell with the animal cell: "The basic granular form again drives home the analogy with the plant, which is manifestly well-nigh wholly made up of granules or cells."

The rapprochement between botany and zoology had come about. The temporary analogy was confirmed when Th. Schwann after a chance meeting with Schleiden in 1839, a meeting which is now famous, published his *Mikroskopische Untersuchungen über die Übereinstimmung in der Struktur und dem Wachstum der Thiere und Pflanzen* and thus made his contribution to the *cell theory* of Schleiden and Schwann.

The anatomical study of the cell gave rise to some differences of opinion. The botanists did not like to abandon the importance which they had given to the cell wall as a primary constituent of the cell—while the zoologists found no cell walls anywhere. It was only in 1833 that von Mohl, as a result of his research into algae and mosses, was able to shift the emphasis from the cell wall to the cell content. In this correct interpretation of the important part of the

cell, he strengthened the connection between botany and zoology.

Schwann had shown that the animal egg was itself a cell and that it was basically like all other cells. Two years later, R. A. von Kölliker showed that the spermatozoon was derived from a cell; hence it was not a mere parasite that lived in the semen. In 1865, O. Schweigger-Seidel and LaVallette St. George demonstrated independently that spermatozoa actually were cells. The cell theory had now reached a point where it could make a major contribution to our understanding of the process of fertilization. But some years were to pass before the process was known in detail.

Historians of biology have universally—and rightly—credited the cell theory to Schleiden and Schwann, yet these two biologists were by no means the first to see cells or to recognize the cells as the basic unit out of which all organisms are built. As early as the seventeenth century, cells had been seen and depicted by Hooke, Grew, and Malpighi. At the time, however, no one realized their full importance. It was not until the beginning of the nineteenth century that a number of biologists, particularly those who worked in Paris, became aware of the fact that animals and plants were made of cells. As we have mentioned previously, Oken had almost grasped the cell theory as early as 1805. Lamarck (1809) had noted that cellular tissue was the matrix out of which organs were formed, and C. F. Mirbel (1808) described different types of cell formation.

It was R. J. H. Dutrochet (1776–1847), however, who insisted for the first time on the individual importance of cells. He had, in fact, been able to study cells intensively as a result of his technique of mascerating plant tissue. He stated, ". . . everything, indeed, in the organic tissue of plants is evidently derived from the cells, and observations have just proved to us that it is the same with animals." His book, *Recherches Anatomiques et Physiologiques sur la Structure Intime des Animaux et des Végétaux et sur leur Motilité* (1824), has a priority of fourteen years over Schleiden and Schwann. His identification of cells, however, was often inaccurate, and he mistook a number of globules for cells.

In 1835, F. Dujardin demonstrated that unicellular organisms are composed primarily of a slimy mass, which he called "sarcode," within which he could observe all kinds of currents and types of motion. Von Mohl found a similar substance inside plant cells in 1846, and stated, ". . . therefore, it may be justifiable for me to suggest in order to describe this substance, a term which applies to a

physiological function, the word Protoplasm." It is true that this word was not original; Purkinjé had used it in 1839 to describe the jelly-like substance of an animal embryo, but von Mohl sharply restricted its application. Finally, in 1850, F. Cohn drew the conclusion that the animal sarcode of Dujardin and the vegetable protoplasm of von Mohl were essentially the same. Thus the concept of protoplasm was consolidated, and the biologists finally focused their attention on the living part of the cell. The living cell, they recognized, consisted of protoplasm. Thus, at last, "living matter" had a name, and this made it accessible for discussion and facilitated its further investigation.

The fact that the importance of protoplasm was not realized until nearly the middle of the century is truly remarkable. Microscopes had been in existence for over two hundred years, and both the cell nucleus and the streaming of cytoplasm within the plant cell had been seen many times. Bonaventuri Corti has described the cytoplasmic streaming in Chara as early as 1774, and two years later he described "la circulation d'un fluide . . . en diverses plantes." In 1776, Felice Fontana also discovered the circular motion of cytoplasm, and cytoplasmic streaming or cyclosis was recorded by Treviranus (1810), Amici (1818), Agardh (1826), and Meyen (1827). Nothing illustrates the "life" of the cell contents quite so spectacularly as this movement of cytoplasm—a movement, incidentally, that is still not explained. Thus, the reason why the importance of protoplasm was not recognized earlier is not at all clear.

The physiologists were foremost in developing and exploiting the concept of protoplasm as "the physical basis of life." The vital activities manifested by plants and animals were found to center chiefly in the processes taking place within the cell. They proved to be inseparable from the activities of protoplasm, and cell physiology soon developed into the physiology of protoplasm. Both colloidal chemistry and physical chemistry participated in developing this central field of physiology, which has now become the special subject of investigation for biochemists who investigate the chemical side of vital phenomena and of biophysicists who deal with the physical phenomena.

The microscopically visible characteristics of protoplasm, such as the granules and fibers, the difference between transparent and opaque protoplasm, the assumed structure of protoplasm as a network of fibers or as a foamy mixture of fluids, gave rise, in the later

nineteenth century, to many—if sometimes unfruitful—debates. The difference between a concept of protoplasm as serving merely a function of nutrition and another concept, which held protoplasm to be an agent that caused the currents and movements within the cell—the movements that might be essential for the transportation of food—were obviously contradictory. The chemical combination of proteins and of numerous other organic substances, the synthesis of organic compounds, and the formation of numerous end-products, were all problems that were not to be solved easily.

The discussions that arose have now either been brought to a satisfactory conclusion or are still acting as stimuli for further research. The nature of the cell as a center of activity in the workshop of the living organism was clearly established in the late nineteenth century, but this threw no light on its nature as a structural unit; for this it was necessary to watch the cell at its inception and to describe it as it came into being.

The chief opponent of the doctrine of preformation, C. F. Wolff, had, in 1759, traced the origin of cells to a jelly-like mass, a *substantia vitrea,* which had as he described it neither shape nor structure. Spaces were formed at various places in this mass. Minute at first, these spaces later expanded until they were separated from each other only by a thin layer. In this way the *vesiculae* (cells) had come into being.

Until 1833, this theory was still accepted by several botanists, the last of whom was Mirbel. Again it was von Mohl who led the research on the origin of cells along the right paths. Out of the incomplete and in part erroneous data of Mirbel, who had taken the formation of spores and the development of buds on a liverwort to be a process of subdivision, he described cell division correctly as a phenomenon of common occurrence. It was von Mohl (1839) who produced the first reliable illustrations of this cell division in the formation of spores, and also in the formation of the guard cells surrounding the stomata in the epidermis of leaves (Figure 48).

At the time, however, von Mohl's contribution was not generally accepted. The cell division that he described was regarded as exceptional. The great stir created by the publications of Schleiden and Schwann on their cell theory resulted in the acceptance of an erroneous explanation of the origin of cells. This explanation, given by Schleiden, bore a close affinity to the earlier explanation of Wolff's. But the views of von Mohl were to have the backing of

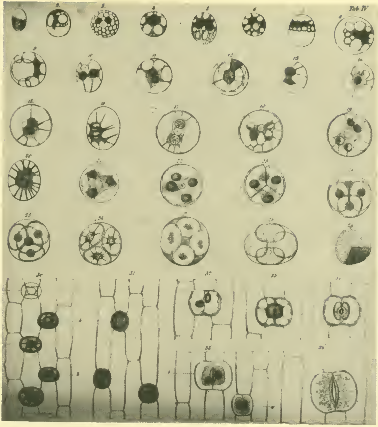


Figure 48. First illustrations of dividing cells. Above, the formation of spores in the liverwort, *Anthoceros*. Below, the formation of stomata. (From von Mohl, 1839.)

other celebrated botanists; and Meyen (1839) and Nägeli (1842) published important contributions in support of the theory that cell division was the principal method by which the cells were generated. Unger, in 1841, was the first to state that all plant cells are formed by the division of other cells.

In the zoological field, also, cell division was studied intensively. In 1841, Remak described the division of red blood corpuscles and other animal cells. Still more descriptions followed; in 1855 Virchow placed his now historical dictum *omnis cellula e cellula* beside Harvey's *ex ovo omnia*.

Meanwhile, in 1831, an important turning-point had been reached in cell research. In all of the cells of the plants he examined, Robert

Brown (Figure 49) discovered a small granular body, the "areola or nucleus of the cell, as perhaps it might be termed." This cell nucleus was destined to occupy a central position in biology and to become the subject of a separate field of general biological investigation—the field of *karyology*. To this discovery Purkinjé and his collaborators added an important extension; that is, that the yolk of birds' eggs contains a small vesicle, *vesicula germinativa*, which was later (1839), but hesitantly, acknowledged by Schwann to be the nucleus. The way was now opened for the recognition of the cell nucleus as an indispensable component of every cell. In fact it was

Figure 49. Robert Brown (1773–1858).

included in the definition of the cell given by Max Schultze in 1861 —i.e., the cell is a quantity of protoplasm containing a nucleus.

Again we must record the fact that the nucleus was observed long before it was found to be a normal component of the cell. Leeuwenhoek himself had seen it in the red blood cells of fish. The artist Franz Bauer (1758–1840) had drawn a picture of it in an illustration that he made of one of John Hunter's specimens. The legend that accompanied the picture stated that it "shows the nucleus." This may be the first time the word "nucleus" was used to describe a cell component. Robert Brown, whose account of the nucleus we have cited, could hardly have been ignorant of Franz Bauer's work. The two men were well acquainted, and for five years, from 1801 to 1805, Brown and Ferdinand Bauer (1760–1826), a brother of Franz, had been shipmates on an expedition that explored the coast of Australia.

The discovery of the nucleus did not immediately have the influence which was its due. Its origin, its function, and its importance were missed entirely. In 1838, Schleiden connected the nucleus with the process of cell division, but he assumed that it was generated spontaneously out of the opaque jelly of the cell. Only after this occurred did he attribute to it the function of attracting to itself a quantity of the jelly, which thus formed a new cell. This assumption was to prevail for a number of years, in spite of the illustrations of cell division shown by Nägeli in 1844 and in spite of Remak's emphatic assertion in 1853—an assertion based on the successive observations he had made on red blood corpuscles for the preceding twelve years. Remak had stated that all cells were formed by the division of a cell after there had been a preliminary fission of the nucleus. Von Kölliker (1845) also asserted that the nuclear division preceded cell division. In spite of all this, such an eminent botanist as Hofmeister maintained as late as 1867 "that the cell nucleus is not capable of individual reproduction."

This refusal of Hofmeister to admit that nuclei came into existence through the division of pre-existing nuclei is one of the oddities in the history of biology. As early as 1847, and again in 1848, he himself had published some exceptionally revealing illustrations of dividing nuclei (Figure 50). When we consider the very crude cytological technique that was available to him we have to admit that his pictures of the successive stages of nuclear division, and even the pictures of the chromosomes that he drew, were truly excellent. It could be that the very details of mitosis that he saw con-

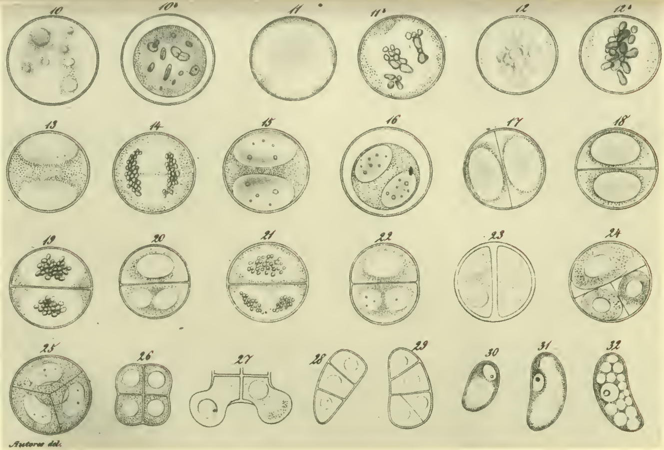


Figure 50. Microspore formation in *Tradescantia virginica*. (From Hofmeister, 1848.)

vinced him that the nuclei as such vanished whenever the cell divided, only to be reconstructed later as the daughter cells were formed.

A major addition to our understanding of nuclear division was made by a zoologist who, except for this one contribution is almost unknown. In 1873, A. Schneider studied cell division in tape-worms, in the course of which he observed a number of thread-like bodies in the nucleus. Thus he was able to give an accurate description of the early stages of nuclear division and of the accompanying cell division. Following this, the development of karyology was advanced rapidly by a number of zoologists; for example, in 1875, O. Bütschli provided a far deeper insight into the process of nuclear fission.

The longitudinal division of the thread-like bodies described by Schneider was confirmed by W. Flemming in 1880 and by E. van Beneden in 1883. Their significance was increasingly recognized, and their detailed structure investigated more intensively. It was discovered that their specific number was fixed in every cell nucleus. This number was characteristic for each species of animal or plant. This was demonstrated in 1885 by the "Zahlengesetz" of C. Rabl and Th. Boveri. The name we now use for these bodies was given them in 1888 by W. Waldeyer. From this time on they were known as "chromosomes." W. Flemming had already used the word "chromatin" in 1879.

The process of nuclear division was now known in outline. Not only did a cell owe its origin to the division into two halves of a pre-existing cell, but the nucleus also was known to split in two before the cell divided, forming two daughter nuclei. Previous to this nuclear division, each chromosome had split longitudinally, producing two half chromosomes. The main lines of *karyology* were now established so firmly that the *generatio spontanea* of cells, nuclei, and chromosomes had been banished forever. The twentieth century thus was freed from an ancient puzzle, freed to delve deeper into the mysteries of the cell nucleus.

Perhaps the most important of the results of this nuclear study was that it led rather unexpectedly to a better understanding of the process of fertilization. It led to an understanding of the physiology of fertilization. Sexual reproduction had long been known as a general—almost universal—phenomenon. The higher and lower animals and the higher and lower plants all produced male reproductive

cells, in addition to egg cells. Fertilization was known to consist of the fusion of the egg with a male reproductive cell. Incidentally, this knowledge had not been obtained easily.

In the eighteenth century, Spallanzani (1786) had filtered the spermatozoa out of the spermatic fluid, but he had assumed, mistakenly, that it was the fluid that fertilized the egg. In 1824, however, Prévost and Dumas demonstrated that it was the spermatozoa that did the fertilizing. In 1857, G. Thuret saw the spermatozoids attach themselves to an egg of the brown seaweed, *Fucus,* and in the next year he showed that the spermatozoids were necessary for its fertilization. Almost simultaneously, G. Newport (1854) saw the penetration of a spermatozoon into the egg of a frog. In 1856, this behavior of the male cell was found to occur in the plant kingdom, when Nathanael Pringsheim (1824–1894) saw a spermatozoid enter the egg cell of *Oedogonium.* In 1877, Strasburger actually saw the male and female nuclei fuse in conjugating strands of *Spirogyra,* but he thought that they followed this fusion by dissolving.

Meanwhile, Oscar Hertwig (1875) had made one of the most fundamental discoveries ever recorded in the history of sexual reproduction. He discovered the role of the spermatozoon after it had entered the egg of the sea-urchin. Others had already discovered that a fertilized egg contains two nuclei which later fuse, thus initiating the development of the young embryo. But Hertwig was able to prove that one of these nuclei had come from the egg cell, the other from the spermatozoon—in other words, that one nucleus was derived from the maternal line, the other from the paternal. Botanical research soon confirmed this: Strasburger (1877) as we have mentioned, found it in the lower and in the higher plants, and E. Maupas (1888) a few years later reported that it occurred also in infusoria. The essential element of the process of fertilization was discovered to be the fusion of a paternal and a maternal nucleus.

This discovery of the basic physiology of fertilization opened the way for a number of different types of experimentation. The influence of the external conditions during fertilization, especially those of the chemical environment, was investigated by Oscar and Richard Hertwig in the period from 1886 to 1889. The possibility of causing an artificial development of unfertilized egg cells by means of chemical stimuli was considered worthy of investigation by J. Loeb (1906) and by others. It was almost imperative that a closer investigation be made of the fate of the chromosomes during the

course of fertilization, especially after E. van Beneden (1883–1887) had affirmed that the chromosomes of the young embryo were derived from each of the fusing gametes—from each of the parents—and from them in equal numbers.

The complexity of the process of fertilization and the striking similarity of the process which was found to exist in all organisms, from the very highest to the lowest, naturally led to a number of theoretical deductions. The subtle mechanism of nuclear division, the meticulous accuracy in the exact splitting of the chromosomes into two equal parts, inevitably gave the impression that it was essential for each cell nucleus to have a precise quantity of the "chromosome-substance." The longitudinal splitting of the thread-like chromosomes even suggested that the division of the chromatin was qualitative as well as quantitative.

These considerations had a marked impact upon a number of unanswered questions. What, for example, could serve as the physical basis of heredity? Whatever it was, it obviously had to have a complex composition and possess some kind of structure. It would also have to pass from parent to offspring in a sufficient quantity and without the loss of any of its essential parts. As the only link between parents and children consisted of the reproductive cells, there followed the biologically obvious conclusion that there must be in these reproductive cells a material "something" that governs the later formation of specific characters.

During the years 1860 to 1890, which saw such an energetic investigation and such an increase in our knowledge of the cell and nucleus, not one of the prevalent explanations of heredity was founded on fact. Unquestionably, the hypotheses that were advanced at the time were of great interest, but none of them was correlated with any observable structure. Herbert Spencer (1863) had assumed the existence of "physiological units" as the material of heredity. Ch. Darwin (1868) had invented his "gemmules," F. Galton (1869) his "stirp," and C. von Nägeli (1884) had assigned hereditary properties to his "idioplasm," but none of them was able to assign these supposed material bases of heredity to a place in the cell. On the other hand the association of these four names clearly indicated the universal nature of the problem of heredity. Spencer was a philosopher, Darwin was originally a zoologist, Galton was a mathematician chiefly concerned with human beings, and Nägeli was a botanist.

All these hypotheses were created before the discovery of the existence and fate of the chromosomes, and none of them assigned a special role to the cytoplasm or nucleus in the transmission of hereditary characters. It must be remembered that, at the time, the doctrine of heredity was still completely speculative; it was in no way a biological science. In contrast to the prevalent speculations stands the prophetic dictum of Ernst Haeckel (1866):

> If we consider, as will be shown below, the shape of every organism to be the product of two different factors, namely of the inherited characteristics in its own substance and of its adaptation to its external environment then these are distributed, in nucleated cells, over the two different substances of the cell in such a way, that the inner nucleus is charged with the transference of hereditary characters, whereas the outer plasma deals with the conformity, accommodation or adaptation to the exterior environment. . . . and then we may rightly regard the nucleus of the cell as the principal agent of inheritance, the plasma as the principle of adaptation.

This statement, dating from 1866, contrasts very sharply with the rather vague theories of heredity prevalent at that time and forms an excellent example of what a genius like Haeckel might feel intuitively without the aid of factual material. Such statements are correct more frequently than we would expect them to be on a purely chance basis. This dictum of Haeckel's also established a working hypothesis, which was to be the starting-point for a new biological science: the science of heredity, or *genetics.*

For the time being, the only factual material that was available consisted of observations on the structure of the cell and nucleus. During the years from 1883 to 1885, most of the eminent figures who, along with their pupils, were engaged in cytological research, followed the path indicated by Haeckel. The zoologist Roux in 1883, Oscar Hertwig, Kölliker, Weismann, and Waldeyer in 1885, the botanist Strasburger in 1884, all followed Haeckel's lead in this attempt to find the material basis of heredity. They concluded that the chromatin threads in the nucleus, i.e., the chromosomes, met the obvious requirements for a material carrier of hereditary characteristics.

Since the embryo received hereditary particles from both parents and since such particles obviously could not continue to double every generation—that is, double every time an egg fused with a spermatozoon—some mechanism would have to exist which would reduce the number of these particles as regularly as it was increased through the process of fertilization.

This mechanism was actually discovered in 1883 by van Beneden, who reported that in *Ascaris* the egg and sperm contained just half as many chromosomes as had the body or somatic cells. Four years later, in 1887, Weismann told how this mechanism would have to work. He stated that there would have to be two kinds of mitoses, in one of which each chromosome would split and form two chromosomes so that each daughter cell could obtain a full complement of chromosomes but, in the other type of mitosis, the chromosomes would have to be reduced in the daughter cells to half of their original number. He even used the terms "equal division" and "reductional division."

The next year, 1888, Strasburger stated that the reduction in the number of the chromosomes in the egg and pollen cells of the flowering plants had taken place earlier in the life cycle, before the pollen grains and the egg cells were formed. He reported that it occurred in the formation of the micro- and megaspores. In 1893, E. Overton discovered that all the cells in the gametophyte of a cycad had only half as many chromosomes as had the cells in the sporophyte, while J. B. Farmer (1894) found the same difference in the chromosome numbers in the gametophyte and sporophyte of a liverwort.

Meanwhile, von Henking (1891) and J. Rückert (1891, 1892) had reported that the double chromosomes in the first reductional divisions were in reality a pair of chromosomes, and Rückert even suggested, although rather hesitantly, that each of the chromosomes that formed a pair was derived from a different parent, and that perhaps there was an exchange of material between these "maternal" and "paternal" chromosomes. At this point it is worth emphasizing that, when Mendelian heredity was rediscovered in 1900, it was no longer a puzzling anomaly. A known material basis was awaiting it—a basis which could account both for Mendelian segregation and for alternate inheritance. By 1901, T. H. Montgomery had collected all the data needed to bring the chromosomes and the Mendelian factors together, and in 1902 W. S. Sutton stated definitely that the chromosomes were the vehicles which transported the Mendelian genes. Later on we shall return to Gregor Mendel (1822–1884) and to his contribution to our knowledge of heredity.

The behavior of the chromosomes also gave the clue that led to the discovery of the mechanism of sex determination. The question as to why some babies were boys while others turned out to be girls had doubtless been asked ever since our progenitors learned how to

ask any questions at all. We have recorded earlier in Chapter 3 some of the highly imaginative answers that were current in classical times. Here we shall review them only briefly.

The fact that there are two sexes and that males have two testes seemed obviously to be more than a chance coincidence. What could be more reasonable than to assume that the semen from one testis (the right) produced males, while from the other (the left) it produced females? And, of course, there was the substitute hypothesis that the semen which fell into the right side of the uterus produced males but that which fell into the left side produced females. A rival and equally reasonable hypothesis was also widespread and just as popular. It was that the parent who was the more heavily sexed would impose his or her sex upon the offspring.

A third notion as to what causes maleness and femaleness strikes us today as being very odd. It was that, when the north wind was blowing, males would be engendered, but that females would be formed when the wind blew from the south. In spite of the fact that this hypothesis could be tested easily and either proven or disproven, it lasted for many generations. Any valid knowledge of what determined the sex of human beings had to await the twentieth century.

The earliest information came from observations that were made on the Hymenoptera—on the family that includes the bees, the wasps, and the hornets. As we have noted (p. 236) Huber in 1792 and Dzierzon in 1845 had recorded the fact that the male bees (the drones) were born from unfertilized eggs, while the females (the workers and queens) came from eggs that were fertilized. Many years would pass, however, before the chromosome basis of this type of sex determination would be discovered. The role of chromosome in the determination of sex can be traced to the work of C. E. McClung.

In 1902, McClung (*Biol. Bull.* 3:43–84) traced the behavior of an "accessory chromosome" (now called the X-chromosome) in the reduction division in the males of the Orthoptera. He reported that this chromosome stood out by itself because it had no chromosome to pair up with. Consequently this chromosome passed into only one half of the spermatozoa that were formed in this division. Inasmuch as he had found this X-chromosome in the males, he assumed very reasonably that the spermatozoa, into which it passed, produced males. In this, however, he was mistaken. In 1905, E. B. Wilson

(*Science* 20:500–502) set the matter right. He found that the female had two X-chromosomes that paired up properly in the reduction division and that all unfertilized eggs received one member of the pair. When such an egg was fertilized by the spermatozoon that had an X-chromosome it developed into a female, as it now had two X-chromosomes. But when it was fertilized by a spermatozoon that had no X-chromosome it developed into a male, as it had but one chromosome—the one it got from its mother.

Some insects have also a Y-chromosome (here defined as the chromosome that pairs with the single X in males). The Y-chromosome in the Diptera, however, seems to be sexually neutral. This follows from the work of C. B. Bridges (*Amer. Nat.* 59:127–137 [1925]; *Science* 72:405–406 [1930]). Bridges showed that the autosomes, i.e., all the chromosomes except the X and the Y, have genes that tend to make the individual male, but that the female genes in two X-chromosomes would overcome these genes and make the individual female. If there were only one X-chromosome, however, the autosomes would overbalance it and make the individual male.

It was only natural that this gene-balance interpretation of the X-Y type of sex determination, the one that Bridges had found to apply to Drosophila, would be applied also to the other divisions of the animal kingdom that had X and Y chromosomes. In fact, for a quarter of a century, Bridges' interpretation was accepted as *the* explanation of the X-Y chromosome control of sex. In 1959, however, W. J. Welshons and L. B. Russell showed that this interpretation did not explain the inheritance of sex in the mouse and, presumably, it would not explain the inheritance of sex in the other mammals—ourselves included. In the mammals that have been investigated thus far, i.e., in mice and men, the Y-chromosome seems to be necessary for the production of males. In the mammals also, normally functioning females may have either one or two X-chromosomes, but if a Y-chromosome is present, a male is produced. In Drosophila, the XXY complex develops a female. In man it develops into an infertile male with the Klinefelter syndrome (McKusick, 1962).

Moths have a very different chromosome complex. In 1906, L. Doncaster and G. H. Raynor (*Proc. Zool. Soc.*, London, pp. 125–133) found in the moth Abraxas that the male had two like chromosomes while the female had the two unlike ones. This type of sex determination was also found in the birds. But the Hymenoptera,

as we have indicated, had yet a third type of sex determination. In 1915, W. Newell (*Science* 41:218–219) reported that, in the honey bee, the males that hatched from unfertilized eggs had only half as many chromosomes (the haploid number) as the females that developed from the fertilized eggs. The females had the double, or diploid, number of chromosomes. Here the balance between the genes in the X-chromosomes and the autosomes could not be responsible for the difference in sex because the balance was the same for both sexes. It was the same in the haploids as it was in the diploids.

The chromosome basis for this third type of sex determination was finally discovered in the parasitic wasp Habrobracon by P. W. Whiting. Here the males are normally haploid and the females diploid as in the other Hymenoptera, but in 1925, together with A. R. Whiting, he proved that some of the males were diploid (*Science* **62**:437–438). Such males could be produced by a very close system of inbreeding, such as mating a mother to her own haploid sons. When this was done, approximately half of the diploid progeny were males but most of the eggs destined to be males did not hatch. Whiting was able to show (*Jour. Heredity* **26**:263–278 [1935]) that, if an individual had a single X-chromosome, or if it had two identical X-chromosomes, it would be male, but if it had two unlike X-chromosomes, it would be female. Apparently no one X-chromosome had all of the factors necessary for femaleness. Finally, in 1940, he demonstrated (*Jour. Morphology* **66**:323–352) that there was a series of multiple allels in the X-chromosome that controlled sex, any two of which when combined would produce a female.

Plants in general present no problem of sex determination since most of the higher plants are monoecious, that is, their flowers are both male and female. But in those species that are dioecious, species in which the plants are either male or female, the problem of sex determination does arise. In 1910, G. H. Shull (*Bot. Gaz.* 49:110–125) discovered strains of monoecious *Lychnis dioica* in a population that was predominantly dioecious. He crossed these monoecious plants with both the male and female dioecious plants. He explained the results he obtained by assuming that the bi-sexual plants were modified males. When they furnished pollen for a cross, their progeny had all the expected properties of the normal dioecious strains. The next year (*Bot. Gaz.* **52**:329–368) he reported

that there were actually two kinds of monoecious plants which differ in the types of progeny they produced both when they were selved and when they were crossed with the dioecious strains. Shull did not examine or count the chromosomes, but he concluded correctly that the male was the heterozygous sex.

It is important that this work of Shull's be recorded in our history because *Lychnis dioica* has experienced a change of name. It is known now as *Melandrium dioicum* and the mechanism of its sex determination has been described under the new name. In 1939, H. E. Warmke and A. F. Blakeslee (*Science* 89:391–392) secured tetraploid plants of Melandrium by treating diploid plants with colchicine. By crossing these plants with the diploids, they secured many different combinations of autosomes, X-chromosomes, and Y-chromosomes. Warmke (*Amer. Jour. Bot.* 33:648–660 [1946]) showed that the autosomes in no way influenced the sex. He also found that neither male plants nor monoecious plants, which of course have male elements, were ever found where the Y-chromosome was lacking, but that male plants were formed when a Y-chromosome was present even when there were as many as three X-chromosomes for every Y. Finally, he discovered that plants that had four X-chromosomes and a single Y developed the bi-sexual or monoecious condition, but even here they occasionally developed into males. The method of sex determination in Melandrium thus is unlike that in Drosophila, where the Y-chromosome is neutral. In Melandrium the Y-chromosome is all important for the production of males, as it seems to be in mammals.

The full significance of the chromosomes as bearers of the Mendelian genes was recognized by Th. Boveri almost as soon as Mendel's work was rediscovered but, strangely enough, some of the leading Mendelian geneticists missed it entirely. They missed it even after they had discovered the transmission of genes that were linked together and after they had recorded the numerical effects of this linkage. In 1904, however, Boveri spelled out the theoretical implications of linkage and pointed out the necessity both for crossing over and for a recombination of the genes (from *Ergebnisse über die Konstitution der Chromatischen Substanz des Zellkernes*, p. 118). This, seemingly, is the earliest recognition of these phenomena:

So we see that along the two lines of research, which have developed quite independently, results have been obtained that agree so completely, that one

line, apparently, can be traced from the other. When we keep in mind what the other facts are that have taught us the importance of the chromosomes for heredity, *it seems highly probable that the characters studied in Mendelian experiments are connected with special chromosomes. . . .* In the same way that the development of double fertilized eggs has shown a difference in the value of the various chromosomes, unconnected with the differences which can be found by visual observations, *the fact that two characters always appear or disappear together, in continuous breeding experiments, allows us to conclude very probably that the material basis for both characters are located in the same chromosome. Further: when hybridization involves numerous characters, while continuous breeding shows that the number of combinations, in which the separate characters may be arranged, is larger than the possible combinations of chromosomes present, the conclusion may be drawn that the characters localized in one and the same chromosome can go to different daughter cells in the reduction division, which would point to an exchange of parts between the homologous chromosomes.*

This prescience of Boveri's deserves emphasis, because he published it two years *before* W. Bateson, E. R. Saunders, and R. C. Punnett attempted to explain the linkage they had discovered in the sweet pea (*Lathrus odoratus*). From 1906 to 1911 (Report III, *Evolution Committee of the Royal Society, Jour. Genetics* **1**:293–302), these scientists sought to explain their results by a complex process of coupling and repulsion. It was not until 1916 that Punnet (*Jour. Genetics* **6**:185–193) accepted the chromosome basis of linkage. Meanwhile, in 1910, Morgan (*Science* **32**:120–122) had found that sex-linked genes were linked to each other and by 1911 he had described crossing over in Drosophila (*Science* 34:384).

The most elaborate cytological theory of heredity in the late nineteenth century, however, was Weismann's, and here again it was the intuition of a genius that was primarily responsible. At first (1885) Weismann had a more or less vague concept of a "germ plasm" which, as a continuous stream of dividing cells, was supposed to link parents and offspring. This was the line of self-producing cells that continued unbroken right through the successive generations. Consequently, Weismann wrote about the "continuity of the germ plasm," the continuity that carried the material of heredity through innumerable successive generations. He tended, though perhaps a little vaguely, to consider the "Schleifen" (chromosomes) as bearers of this germ plasm.

Weismann was not the first to conceive of a continuous line of reproductive cells living in partial separation from the rest of the body (soma) and giving rise to the body cells of each generation. As early as 1849, Sir Richard Owen had described a difference be-

tween the germ and body cells. Gustav Jäger (1878) recognized two basically different types of cells which he labeled "ontogenetic" and "phylogenetic," and in 1880, A. Rauber noticed that only a portion of the fertilized egg developed into the soma. The same year M. Nussbaum described the continuity of the germ plasm. It was Weismann (1885), however, who first saw the full theoretical implications of a continuous and separate stream of germ plasm and who called its existence to the attention of the biological world.

Seven years later, Weismann devised a more detailed hypothesis, in which he stated definitely that this germ plasm—this material of heredity—was in the chromosomes, which he called "idants" (Figure 51). The smallest units of the germ plasm, which Darwin (1868)

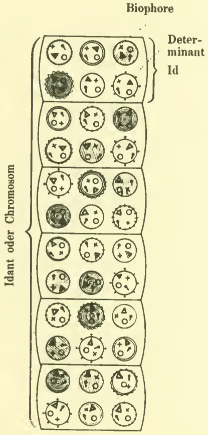


Figure 51. The structure of chromosomes. (From Weismann, 1887.)

had called "gemmules" and de Vries (1889) "pangens," he labeled "biophores," which he considered to be made up of chemical molecules. They were, supposedly, a great many different types of these biophores, depending on the total number of molecules they contained, or on the relative proportion of the different molecules, or even on the chemical constitution and the possibility of isomerism in their grouping.

The biophores that Weismann postulated were by no means mere hypothetical inventions; they must exist, he held, because different kinds of vital phenomena must be linked to differing forms of material units. The biophores would have to be present everywhere in the living organism, in every cell nucleus. Parallel to the so-called "intracellular pangenesis-hypothesis" of de Vries, Weismann assumed that these biophores could pass out of the nucleus at fixed times and play the part he assigned to them in the cytoplasm. For every cell or differentiated group of cells, such, for example, as muscular tissue, he thought a specific group of biophores was responsible. Such a group constituted his "determinants." Supposedly these determinants could exist in the germ plasm or in the chromosomes as single or as composite bodies. In the chromosomes they were localized in definite places and grouped together in "ids"—small disks—which, lying side by side, constituted the "idant" (chromosome).

This complex of hypotheses postulated by Weismann gave a picture of the structure of chromosomes which was completely original. It may be considered a striking proof of Weismann's genius that these hypotheses now form a perfect prototype of the theories to which, seventy years later, genetics still subscribes. This is all the more remarkable because, in Weismann's day, there were no empirical data to support these hypotheses. It should not be forgotten that all the theories and speculations, from Spencer to Weismann, remained without any experimental evidence to substantiate the existence of units in the hereditary substance. These units were the products of sagacious thinkers who made grateful use of whatever cytological observations were available. But they confined their attention to the problem of the localization of the material of heredity. They did not extend their discussions to the hereditary characters themselves or to their passage from parents to offspring.

Heredity itself had been studied; indeed, it had been studied for a very long time. But the results did not appear suitable for a de-

tailed interpretation; or, as often happened, they were lacking entirely. Hereditary characteristics can be examined in every organism, of course, but the technique of investigating them in different groups of animals, and especially in man, is sometimes subject to crippling limitations.

In some cases the number of offspring of a single pair of animals is too small. In others, an otherwise profitable experiment may be rendered impossible because it necessitates the copulation of two selected individuals, who have ideas of their own. With rats or mice this is not a serious problem, but cats are temperamental and sometimes take an unscientific initiative. Often, the problem with human beings also has some non-scientific aspects, which would complicate a purely genetic experiment. In such cases, therefore, research on hereditary genetics can be carried out on only the material that is available. It was Francis Galton's (Figure 52) great

Figure 52. Francis Galton (1822–1911).

merit that he realized this as early as 1869 when he published the first reliable study on heredity in man: *Hereditary Genius, an Enquiry into its Laws and Consequences.* Later, he followed this with his *Inquiries into Human Faculty and its Development* (1883) and *Natural Inheritance* (1889). With these Galton laid the foundations for genetic research through the statistical study of inheritance.

Although Galton may be considered a pioneer in genetic research, it was not possible to establish a general doctrine of heredity on his work or to substantiate his theoretical conclusions by experimentation. That this could and should be done was recognized by the keen intellect of Gregor Johann Mendel (Figure 53), who, by means of well-chosen experimental material, by a careful consideration of

Figure 53. Gregor Johann Mendel (1822–1884).

the problems to be dealt with and, finally, by an exact mathematical analysis of his results, was able in a short lecture he gave on February 8, 1865 to develop the elements of genetics which are still basic to present-day research.

Fate, unfortunately, did not smile on Mendel; his printed treatise appeared in a magazine with a very small circulation. Moreover, the stir created by Darwin's *Origin of Species* was so enormous that Mendel's work escaped notice until it was rediscovered in 1900 by three scientists, C. Correns, E. v. Tschermak, and Hugo de Vries. Hence it was not until the last year of the century that genetics, whose foundation was laid in 1865, earned an equal footing with the other biological sciences. In the twentieth century, its development has been such that it has gained recognition as a separate science in its own right, although it still remains at the very core of general biology.

The thirty-five-year neglect of Mendel and his work needs more than a passing mention. It is true that the biologists of the time were focusing their attention on Darwinian evolution, but they probably were not thinking about evolution all their waking hours, and Mendel was not overlooked entirely. The biologists who read his paper, however, simply did not understand it. Nägeli missed its point even though he had Mendel's personal assistance, as Mendel's letters to him show. Hermann Hoffman (1869) cited and even quoted portions of Mendel's paper, and W. O. Focke (*Die Pflanzen Mischlinge,* 1881) mentioned Mendel fifteen times; but Focke, whose book was the definitive work on plant hybridization, had no idea of Mendel's importance.

This neglect of Mendel really demands our attention, especially when we remember that every single contribution of Mendel's had been made earlier, that some of them were well known, and that all of them were probably known to Mendel himself and possibly to others. But no one before Mendel ever combined the separate elements of Mendelism into a single theory, or realized what the elements revealed when they were properly organized. Even in 1900, when Mendel was rediscovered, some biologists could not follow his reasoning and rejected what they could not understand.

Let us begin with Mendel's precursors! Such unexplained oddities as the sudden appearance of hereditary traits were recorded, of course, long before anyone had any inkling as to what caused them. They were, supposedly, mere *lusus naturae,* and were so described

by practically all of the herbalists. Gerard (1592), for example, described how tulips would not breed true from seed and in so doing, he unknowingly described Mendelian segregation. Tabernaemontanus (1588) and a number of others recorded the fact that different colored grains were born on a single ear of *Zea mays,* and here again we have instances of Mendelian segregation. After plant hybridization was finally recognized by Cotton Mather in 1716, and plants were crossed experimentally, the effects of dominance and recessiveness were recorded routinely and generally without comment. Such instances are really numerous but here we shall confine ourselves to Mendel's precursors who investigated his own genus, *Pisum.*

In 1799, T. A. Knight crossed a white pea with a gray pea and reported that the hybrid was gray (dominant). He back-crossed the hybrid to the white (recessive) parent and secured in the backcross a great variety of peas, including the white. In 1822, the year in which Mendel was born, Alexander Seton and John Goss gave papers before the Horticultural Society of London. Seton had crossed a green pea with a white one. All the peas in the first hybrid generation were green (an instance of dominance) but, when the hybrids were inbred, they produced both types of pea "mixed indiscriminately and in undefined numbers; they were all completely of one color or of the other, none of them had an intermediate tint."

John Goss had crossed a "blue" pea with a yellow-white variety. All the progeny was like the male parent but the second hybrid generation "produced some pods with all blue, some with all white and many with both blue and white seeds in the same pod." Goss carried his experiments through a third generation and reported that the blue peas (recessive) produced only blue peas but that the white peas (dominant) "yielded some pods all white, and some with both blue and white seeds intermixed." He thus described two types of dominants—the homozygous and the heterozygous—as Mendel himself did forty three years later. The next year, Knight reported dominance in the first hybrid generation, and the production of both dominants and recessives when the hybrid was back-crossed to the recessive parent.

In 1826, Augustin Sageret reported the independent assortment of unit characters following a cross he had made between a muskmelon and a cantaloupe, and in 1863, Charles Naudin noted that, in the second hybrid generation, some of the individuals approached very closely to the two parental types. In 1865, B. Verlot found

that certain individuals in a hybrid progeny bred true but that others reverted to a parental form. But none of these botanists made any counts or reported any numerical ratios.

To the best of our knowledge, the only precise Mendelian ratio published before Mendel's own was the one recorded by Johann Dzierzon in *Der Bienenfreund aus Schlesien* (1856). Here he reported that hybrid queens from crosses between German and Italian bees "produced half Italian and half German drones, but strangely enough, not according to the type [not a half and half intermediate type] but according to number, as if it were difficult for nature to fuse both species into a middle race." Dzierzon, as we have stated earlier, had demonstrated that the drones were produced from unfertilized eggs, and thus the one-to-one ratio is just what a Mendelian would expect.

It is almost certain that Mendel was familiar with the work of Dzierzon. We know that he carried on extensive (but unpublished) genetical research on bees. In addition he could hardly have missed the citations of the works of Knight, Seton, and Goss by Carl F. von Gärtner in his *Bastarderzeugung im Pflanzenreich* (1849). Also, to the best of our knowledge, Mendel was the only biologist of his time who both hybridized plants and bred bees. It was Mendel's genius, however, that constructed Mendelism out of the data he found in the scattered publications of the plant hybridizers and in the work of a single bee breeder, data he checked and substantiated, of course, by discoveries of his own—by the facts that he secured through careful and quantitative experimentation. All of the earlier work could only have told Mendel what to look for in his own experiments.

The rediscovery of Mendel was both spectacular and unsettling. In England it resulted in a violent and bitter personal controversy between the Galton school, led by the biometrician, Karl Pearson, and the Mendelians, led by William Bateson. This controversy helps us understand the previous neglect of Mendel. Heredity had been studied for centuries, and it had been universally and correctly recognized that many important characteristics, such as size, strength, vigor, health, yield per acre, etc., were not inherited as simple unit characters. It was not until the researches of H. Nilsson-Ehle (1908), E. M. East (1910, 1913), R. A. Emerson (1913), and R. A. Fisher (1918) were published, that the geneticists were convinced that this so-called "blended inheritance" was in reality Men-

delian and that it could be ascribed to the interaction of polygenes. Again, there was an unnecessary delay in finding a basis for "blended inheritance." As early as 1902, Udny Yule (*New Phytol.* 1:193–207, 223–238) had shown mathematically that this inheritance and the Mendelian could both be accounted for by the same mechanism.

It was not until late in the nineteenth century, long after the time of Mendel, that the behavior of chromosomes was discovered, and a possible physical carrier for the Mendelian factors was found.

As we have stated earlier, both Sutton and Boveri had found the chromosomes to be an ideal vehicle for the transmission of the Mendelian factors from generation to generation. Boveri had even suggested that a chromosome could exchange the hereditary factors it carried with those of its mate. If this occurred, it would account for the observed fact that the number of separate hereditary characteristics was far greater than the number of chromosomes. By this suggestion, Boveri removed a formidable objection to the hypothesis that the Mendelian factors or genes were a part of the chromosomes or that they were attached physically to the chromosomes. Although his suggestion was logical and in accord with the known facts, it was not generally accepted until some years had passed. Of especial interest to historians of genetics is the fact that T. H. Morgan, the first geneticist to locate known genes in recognizable chromosomes, rejected the hypothesis completely. He did not accept it until his own discoveries had forced him to reverse his stand.

Perhaps the most significant aspect of the following passages lies in the fact that Morgan published them six years after Boveri had indicated that homologous chromosomes could exchange Mendelian factors, and four years after Bateson and Punnett has discovered an actual instance of genes travelling together in groups but separating sometimes and travelling apart. The date of Morgan's publication is August, 1910. The month is important. From *Amer. Nat.* 44:449–496:

> Since the number of chromosomes is relatively small and the characters of the individual are very numerous, it follows on the theory that many characters must be contained in the same chromosome. Consequently many characters must Mendelize together. Do the facts conform to this requisite of the hypotheses? It seems to me that they do not. (p. 467.)
>
> But even admitting this possible way of eluding the objection, the other point raised above concerning the absence of groupings of characters in Mendelian inheritance seems a fatal objection to the chromosome theory, so long as that theory attempts to locate each character in a special chromosome. (p. 468.)

Our general conclusion is, therefore, that the essential process in the formation of the two kinds of gametes of hybrids in respect to each pair of contrasted characters, is a reaction or response in the cells, and is not due to a material segregation of the two kinds of materials contributed by the germ cells of the two parents. (p. 479.)

On the other hand, there is no need to assume that X is the *sex chromosome* in the sense of carrying sex. (p. 494.)

But for Morgan, the road to Damascus was not long, and his conversion, when it came, was complete. He had seen the light even before the passages cited above got into print. A month earlier (in July), he had described sex linkage in Drosophila, and had shown how a gene for white eyes was inherited along with the sex factor (*Science* 32:120–122). In the following October, he gave a talk in which he told how two sex-linked characters were inherited together in the same animal (*Proc. Soc. Exp. Biol. Med.* 8:17–19), and in an address he gave in December, he came about full circle and reversed himself completely (*Amer. Nat.* 45:65–89 [1911]):

. . . the more serious objection still remained, namely, that with a small number of chromosomes present, many characters would Mendelize together, but very few cases of this are known. De Vries was the first, I believe, to point out that this objection would be met if the genes are contained in smaller bodies that can pass between homologous pairs of chromosomes; and Boveri has admitted this idea as compatible with his conception of the individuality of the chromosomes. In the case of the inheritance of two sex-limited characters in the same animal we have an experimental verification of this hypothesis. (p. 78.)

In September, 1911 Morgan made the important generalization that those genes that were segregated at random were carried in different chromosomes, but that those that were linked together were carried in the same chromosome. In this paper he also suggested that the genes that were closely linked were physically close together in the chromosome, while genes that parted company a large part of the time were further apart (*Science* 34:384).

The actual location of Mendelian genes on specific chromosomes followed shortly. It was the work of Morgan and of his co-workers, A. H. Sturtevant, C. B. Bridges, H. J. Muller, *et al.* Morgan (1910) reported that, in the little fruit fly Drosophila, a white eye and also a yellow body color were passed from generation to generation just as the "X" or sex chromosome had been. Their inheritance was "sex-linked." In Drosophila, just as in human beings, the male has one X-chromosome and the female two. Thus if a gene in an X-chromosome is a Mendelian recessive, the female may carry it and

pass it on to her offspring but need not show it herself. The male, on the other hand, having but one X-chromosome cannot cover it up, but must show it. Sex-linked heredity had been found earlier in the moth Abraxas by Doncaster and Raynor in 1906, and Spillman, in 1908, had shown how sex-linked inheritance in birds followed this scheme. But here, as we have stated, the female has but one X-chromosome while the male has two. This type of sex-linkage had been seen much earlier but, of course, not understood. It had been described in chickens by E. L. Layard (*Ann. Mag. Nat. Hist.*, 1854) and also by Samuel Cushman (*Ohio Poultry Journal*, 1893). But Drosophila had the same type of sex linkage that human beings had.

Immediately, this discovery explained an anomalous type of human inheritance that had been known for a long time. In 1777, Joseph Priestley and in 1778, Michael Lort described the sex-linked inheritance of color-blindness. In 1798, the great chemist, John Dalton, who had tried to do research on the spectrum but who had discovered that he was color-blind, investigated his own deficiency so well that, for a time, this sex-linked character was known as Daltonism. In 1793, the sex-linked inheritance of haemophilia was described anonymously in the *Medicinische Ephemeriden of Chemnitz* and in 1803 by John Otto of Philadelphia. In the nineteenth century other instances of sex-linked heredity were described—e.g., night blindness (1878) and nystagmus (1882)—but, of course, without any understanding of its cause.

Today, genetics has assumed the typical form of a concentrated science. It has elucidated many old problems and revived in a new form the controversy between preformation and epigenesis. This has now developed into a controversy between those who emphasize the predetermined factors in heredity and those who emphasize the later influences exercised by the environment and the living conditions. Although genetics places the main emphasis on heredity, it is fully aware that, in its effect on the individual, heredity is rarely uninfluenced by the external conditions—by the environment of the individual. It understands that every organism, whether man, animal, plant or microbe, is intrinsically the product of two groups of factors: (1) heredity which is the realm of genetics, and (2) the environment which as a field of study belongs to a different branch of biological research.

Genetics now fulfills a function in biology which, in its far-reaching influence, encroaches on almost all fields of biological re-

search. It impinges on microbiological, botanical and zoological research and on the fields of anatomy, embryology, physiology, and systematics. Moreover, it extends beyond the domain of biology proper and is concerned with medical and agricultural questions and even with problems in the life of the community—with jurisprudence and even with philosophy. For its own purposes, genetics seeks to collaborate more and more with other biological and even with certain non-biological sciences. Today, it must cooperate with cytology and, in the investigation of the process of development, with biochemistry and physiology. It must also join forces with colloid chemistry and with x-ray physics. It has combined with mathematics to form biometrics. Genetics found its basis in Mendel's experiments, in Galton's genealogical research, and it gratefully accepts the cytological observations that the German biologists made around 1880. The science, christened *genetics* in 1906 by its protagonist W. Bateson, has now come of age.

Systematics, especially the systematics of animals, acquired a new dimension at the time of Lamarck. Besides the basic classification, which aims only at bringing some order into the chaos of the animal and plant kingdoms, and pigeonholing and labeling all its varieties, a new trend appeared in the field in the pursuit of affinity. This "affinity" was soon looked upon as a "relationship," and the recognition of a supposed "relationship" led to a suspicion of a real relationship—a blood relationship or the relationship of a common descent.

Arguments were advanced from all sides, arguments that postulated that the flora and fauna, as we know them today, had changed in the course of the earth's history, that different plant and animal forms used to inhabit the earth, and that even now the world of organisms is not constant. In Linnaeus' day, it often proved difficult to draw a definite line between two species. One species was found to be multiform, while another was concentrated round one particular type. Doubts began to arise as to the generally accepted immutability of species. Linnaeus himself had seen that new species might still arise at any time, but he was also aware of the great obstacles which this liberal doctrine placed in the way of his greatly needed classification. He expressly confined himself to the dictum: "Species tot numeramus, quot ab initio produxit infinitum Ens"—"There are as many species as were created by the Infinite Being from the beginning" (1737).

But the size of the natural history collections in museums and herbaria were increasing daily, and the richer the collection became the harder it was for the biologists to deny the existence of "varieties" as subdivisions of their "Species." Lamarck, in his *Philosophie Zoologique* (1809), was one of the first to express doubts as to the immutability of species:

The name species has been given to every collection of similar individuals which have been produced by other individuals like themselves. This definition is correct. . . . But to this definition has been added the supposition that the individuals which make up a species never vary in their specific characters and that consequently the species has an absolute constancy in nature. It is exactly this supposition that I propose to combat.

At this point it might be well to insert a fact that has generally been overlooked by the historians of biology. The pre-evolutionary concept of species is generally given as a universally accepted view that species were constant and true breeding forms. Actually, the idea that species were completely stable and unalterable units had dominated biological thought for only about a hundred years when Darwin attacked and shattered it. All during classical and medieval times, species were looked upon as something specious. The species of an object were only its appearances and, proverbially, appearances were deceitful. Supposedly, species were only temporary forms and they could change into each other whenever the occasion demanded. Wheat could "degenerate" into barley, and barley into oats. Promiscuous and unsystematic hybridization supposedly could also produce new species. Both animals and plants had been described as changing their species whenever they were transported to new countries. But this widely accepted concept that species were only ephemeral and mutable units did not lead to a belief in evolution.

Before a belief in an orderly and systematic evolution could become respectable, the relative stability of species would have to be established. And, in the eighteenth century, as the result of careful and accurate research in the field of systematics, this view of species did supersede the older one. But this careful research also showed that the stability of species was not absolute. There was evidence that some species had become altered, even if only a little bit. It was the secular accumulations of this residual instability in otherwise stable species that led to the theory of evolution.

Lamarck's concept of inconstant species was, on the whole, com-

patible with the facts discovered in the early nineteenth century. These discoveries lent support to the theory that, in the course of time, the flora and fauna of the entire earth had changed markedly. Moreover, it became clear that there was a close "relationship" between some groups of organisms and a very distant one between others. Extinct species, by differing from those that were living, showed that the world of life was not static.

The study of fossils so greatly encouraged by Cuvier was held by Lyell to be a necessary part of geological investigation. The development of the earth was divided into eras extending over 550 million years—eras which showed a succession of stages—a succession of strata—in which the plant and animal remains were imbedded. Sometimes they were found in abundance, sometimes in mere traces. The fauna of the oldest layers, the so-called "precambrian," was exceedingly sparse. In the successive layers the number of fossils gradually increased; at first they consisted merely of simple animals and of a few simple plants. On the whole, the complete palaeontological record of the flora and fauna followed the same general trend. The animals and plants in the oldest strata were far more primitive and far simpler in composition than were the later groups of organisms.

As the century progressed, arguments for the mutability of the species came also from a number of other sources. Comparative anatomy, which began to develop rapidly about 1800, indicated that there were analogies in structure between large and separate groups of species. It also became clear that the *embryological development* of the so-called "higher" animals showed a number of stages reminiscent of the more primitive forms. During its embryological history from fertilized egg cell to mature individual, every mammal passed through a phase where it had gill-slits and a tail and these structures were suggestive of the fish. In a subsequent period of development these fish-like features were replaced by those of a terrestrial animal. This principle, found not only in the mammals and other vertebrates but also in many invertebrates, had already been recognized by Meckel. Later in the century, as we have stated (p. 249), it was elevated by E. Haeckel to the status of the "biogenetic law."

All of these ideas and discoveries centered around the network of problems that were to form the subject-matter of organic evolution. Such were the problems of the constancy or mutability of biological species, the question of the extent to which the line of

descent of the evolving species showed a trend from "lower" to "higher," and, above all, the most difficult and also the most important question as to what were the causes of the mutability of species. What caused them to change in their mutual descent and in their evolution from the primitive to the complex? These problems had already attracted the attention of the ancient Greek philosophers; but now, fortified and enriched by observations and comparisons, they assumed a more scientific character. The problems also became much more insistent.

Again, it was Lamarck (Figure 54) who, in his *Philosophie Zo-*

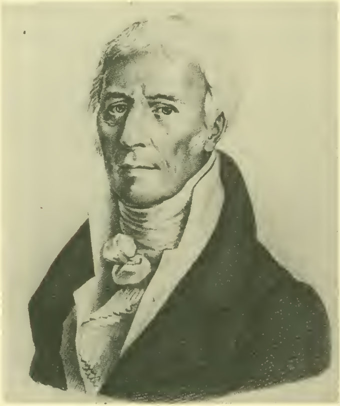


Figure 54. Jean Baptiste Pierre Antoine de Monet, Chevalier de Lamarck (1744–1829).

ologique (1809), made the first attempt to trace the causes of the mutability of species and who formulated two laws—the "First law: In every animal which has not exceeded the limits of its develop-

ment, the more frequent and continuous use of one particular organ, gradually strengthens, develops and enlarges it, and gives to it a power proportional to the duration of this employment; whereas the progressive disuse of such an organ weakens it little by little, attenuates it, progressively lessens its faculties and finally causes it to disappear altogether," and the "Second law: All that Nature has caused an individual to acquire or to lose through the influence of an environment unto which its race has long been subjected, and accordingly through the constant use or disuse of a particular organ, is preserved by Nature by transmission to the new individuals which are born, provided the acquired alterations are common to both sexes or to those which have produced the new individuals." Thus Lamarck advanced an hypothesis that was capable of explaining both

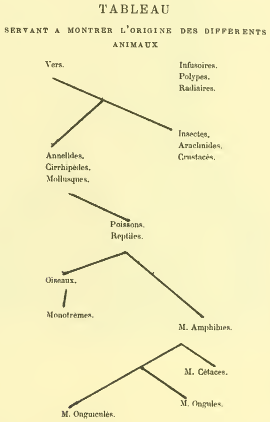


Figure 55. Lamarck's scheme of evolution (M = mammals).

the mutability of species and the evolution of species from lower to higher (Figure 55).

However, this hypothesis also touched upon the vague subject of the "heredity of acquired characteristics." This subject was to give rise to a vehement controversy and discussion long after Lamarck's day; but it has now, at any rate as far as his somewhat naive definition is concerned, been brought to a negative conclusion. Lamarck's theories were frequently ridiculed, sometimes unfairly, both in puns and in caricatures: Caran d'Ache formulated his replies to the questions of M. Toto in a series of witty drawings (Figure 56), and Lord Neaves wrote:

> A deer with a neck that was longer by half
> Than the rest of his family's—try not to laugh—
> By stretching and stretching became a giraffe
> Which nobody can deny.

The theory of evolution was not fortunate in having Lamarck as its first great advocate. Lamarck undoubtedly was a genius, but he was an erratic genius, as outstanding in his limitations as in his accomplishments. He could never grasp the simplest chemical concept; for example, he thought it absurd for Lavoisier to believe that oxygen was an essential component of both air and water. But Lamarck had the courage of his convictions and he never hesitated to commit himself. He attacked the "pneumatic chemistry" of Lavoisier, and he seemed to be unable to grasp even the simplest physics, as he denied that air could convey sound. He never knew when he was making himself ridiculous for he apparently had no sense of humor whatever.

In fact, where most men have a "bump" of humor, Lamarck had a positive depression. The explanation he gave of the origin of the giraffe's neck was only asking for ridicule. He told how the crane got long legs by stretching them as it waded in water and how the gander got a long neck by extending it when he fed on the bottoms of small ponds. The bull got horns, he said, because he was short-tempered and, whenever he lost his temper, his blood rushed to his head and there deposited small bits of bony and horny matter.

Even though the evidence for evolution was accumulating rapidly during the first half of the nineteenth century, few biologists wished to be called Lamarckians and be laughed at. At the time, evolution was not so much in the air as it was underground. However, it remains Lamarck's great merit that he was the first to have attested

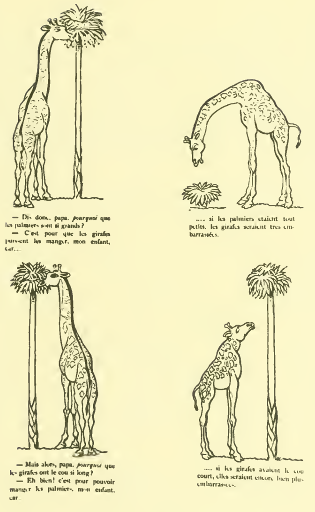


Figure 56. Caricature of Lamarck's theory of evolution by Caran d'Ache.

with certainty that "species" as defined by Linnaeus, were not constant. And in this respect the words of his daughter, to her blind, disappointed father, "Posterity will honor you," have decidedly come true.

To combat the elderly Lamarck, who was defended by the rather timid and hesitant efforts of Étienne Geoffroy St. Hilaire, the much younger, pugnacious, and confident Georges Cuvier (Figure 57) entered the field. Starting from his knowledge that each stratum

Figure 57. Georges Cuvier (1769–1832).

contained a specific type of fossil fauna and that the differences between the fossils from the different strata were as great as that between the more recent fossils and living animals, he concluded that the flora and fauna in each phase in the geological history of the earth were destroyed by great catastrophes. These catastrophes, he held, were far more drastic in their effect than were the more normal processes which from time to time still affected the surface of the earth. He was convinced that a series of such catastrophes or cataclysms, each one followed by a new creation, had occurred during the past history of the earth. It was only after the latest catastrophe that man, together with the recent animals and plants, had been created.

This belief in catastrophes was generally accepted in France, which at that time set the fashion in science. In a debate between Cuvier and Geoffroy St. Hilaire at the Paris Academy in 1830, the

permanent triumph of the belief in catastrophes seemed assured. Yet, in the very same year, Ch. Lyell gave such convincing proof of its untenability in his epoch-making *Principles of Geology* that the belief in catastrophes was soon discarded. It soon became a mere historical curiosity. Charles Lyell's "uniformitarianism" took its place.

Following the triumph of uniformitarianism in geology, the way seemed cleared for the acceptance of some sort of doctrine of evolution in biology. During the first half of the century, data were accumulating that could be explained easily by the assumption that species had evolved, but they could not be explained if species were unalterable. The theory of evolution, however, remained in disrepute. Even such scientists as Lyell and Huxley rejected it at first. But the "developmental hypothesis," as it was called, was known and discussed widely, even outside of biological circles. Evolution may have found its greatest popularity among non-biologists, at least in the English speaking countries. The reasons for its popularity may have been due to an historical accident.

In 1844, a book entitled *Vestiges of the Natural History of Creation* was published anonymously. This book supported the doctrine of evolution. The work was well and clearly written and it passed through many editions. Its tone was pious, its arguments eloquent, and, to anyone not too well acquainted with biology, its reasoning was very convincing. Its author was the Scotch publisher, Robert Chambers. Unfortunately, the *Vestiges* contained so many biological errors and it based its conclusions on such dubious data that it repelled almost every biologist who read it. Next to Lamarck's unconsciously humorous arguments for evolution, it did more, perhaps, than any other single work to relegate the doctrine of evolution to the lunatic fringe. But it did counter some of the religious opposition to evolution, and it did inform the reading public of some of the data on which evolution was based. Evolution, however, needed an intellectually respectable explanation and this Charles Darwin gave it in 1859 (Figure 58).

Darwin had been convinced of the truth of evolution some twenty years before he published his famous work, the *Origin of Species,* in which he gave his explanation of what made species evolve. He had delayed the public announcement of his belief while he was amassing an enormous amount of evidence to substantiate the fact that evolution had taken place. He was finally stimulated into present-

Figure 58. Charles Darwin (1809–1882).

ing his work to the public by the fact that his young friend, Alfred Russell Wallace (1823–1913), had worked out independently the same explanation that he had. This explanation, incidentally, is still basic to our understanding of evolution.

Both Darwin and Wallace were world travellers and both got their ideas of evolution in the tropics. Darwin had travelled round the world from 1831 to 1836 by way of the Southern Hemisphere, while Wallace had lived in Brazil from 1844 to 1848 and roamed through the Malay Peninsula during the years 1854 to 1862. Both scientists returned enriched by their experiences, and both were impressed especially with the remarkable distribution of species throughout the island groups. To Darwin came the revelation of

the Galapagos Islands, each of which had its own species of plants and animals. These species had an easily-recognized affinity with those on the continent of South America, yet each island had its own indigenous species each with its distinct characteristics. To Wallace came a similar experience in the Malay Peninsula.

Immediately on his return in 1836, Darwin began to tackle his mass of material and notes, in the course of which he came to an entirely new conclusion. In 1842, he wrote a short essay of 35 pages which he elaborated in 1844 into a treatise of 230 pages, but he did not dream of an immediate publication. Neither of these works was published until 1909, the hundredth anniversary of Darwin's birth.

Wallace, for his part, had written a short article at Sarawak in 1855 entitled "On the law which has regulated the introduction of new species" and in 1858, from Ternate, he wrote a paper, "On the tendency of varieties to depart indefinitely from the original type." He sent the latter as a preliminary sketch to Darwin, who found in it much that corresponded with his own views. Darwin immediately wished to publish it in Wallace's name, but was finally persuaded by the insistence of his friends Ch. Lyell and J. D. Hooker (1817–1911) to write an exposition of his own ideas, and publish it along with Wallace's article. Darwin's contribution to the joint presentation, which was made at the meeting of the Linnaean Society on July 1, 1858, was a short selection from his manuscript of 1844 and an abstract from a letter he had written to Asa Gray on September 5, 1857.

Thus it happened that circumstances, including the urging of his friends, compelled Darwin to present his evidence for evolution to the public. He immediately started work on a more adequate statement of his views, and in so doing he abstracted the book he had been working on for so long. This abstract was published on November 24, 1859. It was the now famous *Origin of Species.*

The possible conflict between Darwin and Wallace over priority was not concerned with the fact of evolution but over its explanation. Incidentally, both scientists had found their clue as to what caused evolution when they were reading Malthus' *Essay on the Principles of Population.* Darwin and Wallace had discovered the basic factor in evolution that is designated by the term "natural selection."

Since this factor is still recognized as the *sine qua non* of evolu-

tion, it would be well to state it briefly. This can be done most economically by a short series of propositions: (1) All organisms vary, no two are ever alike. (2) More are born than can exist in the available space. (3) Consequently, there is a keen competition between the individuals of every species (The struggle for existence). (4) The weakest or the least fit are eliminated. (5) The fit or better adapted survive. (6) The survivors form a basis for new variations. (7) The variations selected long enough produce new species.

We honor Darwin and Wallace chiefly because they saw the full implications and consequences of these propositions. They were not the first, of course, to recognize the propositions separately, nor were they the first to understand the workings of natural selection as a whole. As we have stated, in classical times, Empedocles and Lucretius had described the non-survival of the unfit and had used this aspect of natural selection to explain the existence of adaptation and as a rival hypothesis to teleology. In the eighteenth century, this naturalistic explanation of the existence of adaptation was given by Diderot (1749) and Maupertuis (1756), and in the nineteenth century by Geoffroy St. Hilaire (1833) and by Darwin's good friend, Edward Blythe (1835–1837). The struggle for existence had been described by Al-Jahiz in the ninth century and by Thomas Hobbes (1651), Sir Matthew Hale (1677), Buffon (1751), Lord Monboddo (1773), Kant (1775), and others.

The role of natural selection in evolution had been envisioned in part by J. C. Prichard (*Researches into the Physical History of Man*, 1826). Prichard had expressed the greater part of his ideas in the earlier editions of 1808 and 1813. He explained how the different human races had become different and how they had become adapted to the different climates and indigenous diseases of the countries where they lived. He stated simply that the unadapted individuals and groups who had invaded climatic zones to which they were not adapted had already perished. Contemporaneously with Prichard, William Lawrence (*Lectures on the Natural History of Man*, 1819) discarded the hypothesis of the inheritance of acquired characters, and held that human adaptation was secured through the selection and propagation of spontaneous variations that were heritable. In 1813, William Charles Wells explained how the differences between the several human races had arisen through natural selection and, in 1831, Patrick Matthew (*Naval Timber and Arboricul-*

ture) accounted for the fitness of species as arising through an over-production of young, followed by a struggle for existence and the destruction of the unfit. In 1852, Herbert Spencer and C. V. Naudin showed independently how population pressure followed by a differential death rate would alter varieties and species. Finally in 1858, J. H. Klippart (*Annual Report, Ohio State Board of Agriculture*) demonstrated that a more fit (prolific) race of wheat would replace a race that was less fit, if the seeds were mixed and the two races grown in the same fields. This was the year in which Darwin and Wallace gave their joint paper.

Darwin's theory of natural selection accounted for the origin of species through neither special creation nor through the inheritance of acquired characters, but through the selection by nature in all of its aspects and manifestations—through geographical and sexual selection, through the "survival of the fittest," etc. For new species to arise it was only necessary for the old species to vary and for the variations to be heritable.

This theory caused a great stir both among professional biologists and among amateurs who were engaged in biological research. It also received fierce and sometimes unfair opposition, particularly from certain theological quarters. This was counterbalanced by the support of Darwin's personal friends, Ch. Lyell, J. D. Hooker, A. R. Wallace, Asa Gray, and T. H. Huxley, and by the popular and at times overenthusiastic propaganda of Ernst Haeckel. Darwin's theory also became a butt for the writers of epigrams: Miss Kendall puts the following lines into the mouth of a prehistoric Ichthyosaurus:

> Ere man was developed, our brother
> We swam, we ducked and we dived,
> And we dined as a rule, on each other,
> What matter? The toughest survived.

Yet in the end, Darwin triumphed. His theory as a whole has proved invulnerable, even if it has here and there been revised by modern research.

What Darwin was unable to tell us was the cause of the inherited variability from which Nature makes her selection. This remained unexplained in his time and, even now, it has no complete answer.

The publication of the *Origin of Species* initiated what must have been one of the most enjoyable scientific disputes of all time. Prac-

tically anyone could get into the fight. At first the opposition came from those biologists who were unable to alter what they had been believing for years. They found allies almost immediately among the theologians and, in England, even among the politicians. No knowledge of biology was required of those who participated in the controversy—indeed, they might have found such knowledge to be inhibiting. And this aspect of the affair added greatly to the gaiety of nations.

Among the technical biologists, however, evolution soon triumphed completely. But outside of biology proper, and even within the science, opposition to the origin of species through natural selection has persisted. To many, the struggle for existence just seemed too brutal, and a nature that was "red in tooth and claw" seemed incompatible with a humane universe. Many able biologists simply could not face all this harshness. We may cite among those who belittled or denied natural selection, Alfred Giard (1846–1908), E. D. Cope, O. Hertwig, Theodore Eimer (1843–1898), Richard Semon (1859–1919), and August Pauly (1850–1914).

Even today—a century later—this opposition to natural selection persists, but now it persists chiefly in uninformed circles. Even so, this particular misunderstanding of evolution and what it implies is still being propagandized. To counter it, we might include here a brief post-Darwinian history of evolution even if we have to come down to the present and trespass into the jurisdiction of some of our current biological textbooks.

Granting the existence of heritable variations as data—as something given by observation—natural selection can explain the preservation of useful novelties and the destruction of those that are harmful. But natural selection cannot explain the evolution of neutral characteristics nor the ultimate degeneration of all organs or functions that have lost their utility—such a degeneration as the eyes of cave fish which can no longer see. To explain how this blindness arose through natural selection, we would have to assume that, in the total darkness of caves, eyes that could not see under any conditions were somehow or other more fit than eyes that could see only in the light.

Fortunately for the rationale of evolution, there was an ancillary explanation already in use—i.e., the inheritance of acquired characters—and there was nothing antithetical between natural selection and this alternate explanation of evolution. Darwin himself ac-

cepted both explanations. Indeed, in the later editions of the *Origin*, he relied more and more on the latter explanation. It was not until the last two decades of the nineteenth century that biologists had to abandon the inheritance of acquired characters, and this threw the entire burden of explaining evolution on natural selection—a burden natural selection could not bear without the support it was later to receive from Mendelian genetics. But from about 1890 to 1910 the explanation of evolution was not satisfactory.

It was clear, however, that evolution always depends upon heritable novelties—novelties that are able sooner or later to displace the earlier types from which they sprung. Thus, to understand what caused evolution, it was necessary to know how new factors could arise from time to time, and how they could be inherited. This, of course, could be learned only through empirical, experimental research. It was the unforgettable achievement of Hugo de Vries (Figure 59) to have understood this basic principle. De Vries brought some plants of *Oenothera Lamarckiana* into his experimental garden because they had shown a remarkable degree of variability, and this variability was obviously a favorable subject for an evolutionist to investigate. From 1886 to 1899, he raised a total of 55,000 individual plants and discovered among them 834 mutant forms. He found that the same mutation occurred over and over again and that the mutants fell into but 7 different, true-breeding species.

De Vries had actually observed evolution in action. The mutations he saw were real. They were jumps—real saltations. His new species were all formed in a single step and from a single mutation. They had come into being without any long-drawn-out and tragic struggle for existence between individuals who differed only in small Darwinian variations. Thus it was only natural that the "mutation theory" of de Vries would be looked upon in the popular mind as a more humane rival to the theory of natural selection.

De Vries' conclusions, however, had a checkered career. At first, they took the biological world by storm—they were *the* solution to the evolution problem. But, as other genera were investigated, it was found that *Oenothera* was not typical. It was found by experimental studies around 1910 that *O. Lamarckiana* is not a true breeding species but a very complex hybrid (Heribert Nilsson, O. Renner, R. R. Gates, *et al.*). The matter was not cleared up until 1924, when R. E. Cleland showed that the chromosomes of *Oenothera* had a

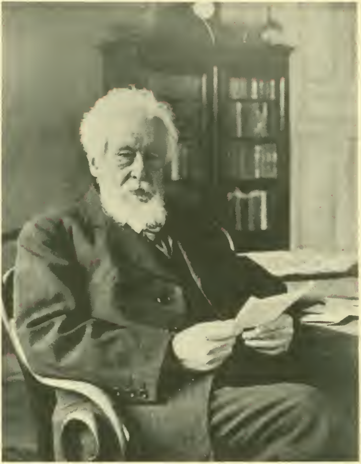


Figure 59. Hugo de Vries (1848–1935).

highly individualistic behavior pattern and that *O. Lamarckiana* was indeed a true-breeding hybrid which occasionally reverted to some other (perhaps ancestral) type. Meanwhile, Mendel's forgotten work had been rediscovered (de Vries himself was one of the discoverers); mutations were found to occur and they turned out to be hereditary. But they were not always able to produce new species in a single saltation.

We know now that mutations do not have to be as drastic as they seemed to be in *Oenothera*. They may be as small as a change in a single Mendelian gene (a point mutation), or they may involve a section of a chromosome (as occurs sometimes in Drosophila) or a

whole chromosome (as in Datura), or a set of chromosomes (Nicotiana) or we may even have polyploidy running wild (as in *Crepis* and in some of the ferns). Mutations can be either large or small, but even point mutations can be so drastic that no investigator could miss them, or their effects might be so small that no investigator could identify them individually. We know now that mutations are the building blocks of evolution and that they themselves are the Darwinian variations that nature selects.

Soon after mutations were incorporated into the evolution picture, however, an unexpected difficulty arose. Mutations seemed to occur with extreme rarity. Also, practically all of the mutations that were observed turned out to be detrimental. The most common of all mutations were lethals, and only a cynic would hold that such mutations would lead to evolutionary progress. It was soon evident that mutations by themselves could not explain evolution but, unless they were checked, they would sooner or later lead a species to extinction.

A considerable time elapsed before it was realized that the mutations that were the subject of genetical research were not typical of mutations in general. In fact the geneticists had selected their mutations—the ones that they investigated—just as ruthlessly as nature had selected a different type of mutation in the past. All mutations, obviously, were not equally obvious; and geneticists found it much easier to identify and handle drastic mutations than mutations that were insignificant. Naturally, small mutations were often overlooked. Small mutations also turned out to be much more frequent than was generally realized, and all such mutations, it was discovered, need not be harmful. Recently, the occurrence of beneficial mutations has been observed.

We do not yet know just how frequently these small mutations occur. Some estimates place their frequency as high as a hundred times that of the large destructive mutations. That they are frequent, however, has been shown by E. Baur (*Bibliotheca Gen.*, 1924), T. Tammes (*Zeit. ind. Abs. u. Vererb.*, 1925), and E. M. East (*Amer. Nat.*, 1936). In recent years, mutations beneficial to insects have produced new strains that are immune to insecticides, and strains of bacteria have arisen, unfortunately in our hospitals, that are immune to antibiotics. There is now no need to discard mutations as a major factor in evolution, although there are some few who do so.

W. Johannsen also undertook to study evolution quantitatively.

In 1903, he published the results of his selection experiments on beans. From out of a general population, he selected beans of different sizes and bred from them. (Beans are generally self-fertilized.) He found that these differences in size were inherited and that each bean produced a population that differed from the other populations. He continued to select for size within each of these sub-groups, and this time he discovered that the differences in the size of the beans within the sub-groups were *not* inherited. In this latter instance, he was merely selecting within a pure (homozygous) line. At the time, it was practically inevitable that these non-heritable variations would be labeled "Darwinian" variations and the fact that they were not inherited would be contrasted with the heritable mutations of de Vries. Some time elapsed before the misunderstanding was cleared up. Variations within a homozygous line have to be environmental in origin. They are Lamarckian and, of course, are not inherited.

Still another factor in evolution, one especially noteworthy in the plant kingdom, is the spontaneous hybridization of species that takes place in nature. In this way, many new species have been produced. The hybrid origin of many new, true-breeding species has now been established in *Triticum*, *Nicotiana*, *Oenothera*, *Crepis*, and a number of other genera; and the alteration of a species by wandering genes from related forms (introgression) has been found in *Crataegus*, *Viola*, *Quercus*, etc. We also have one famous instance of a new genus (*Raphanobrassica*) being formed artificially, when a radish was crossed with a cabbage, and the chromosome numbers in the hybrid were doubled. Hybridization is a source of much variability and of many new combinations of factors. Its importance was emphasized by the "hybridization theory" of J. P. Lotsy (1916).

Finally we have the sampling error, sometimes called the "Sewall Wright effect." This explains how useless characters often become fixed in isolated groups. In small populations or breeding groups, some Mendelian genes may be lost purely by chance. It may even be the more effective form of the gene that disappears. We should hardly expect neighboring but isolated groups to lose exactly the same genes. Thus, mere isolation can cause groups to become different, but in large populations this "sampling error" becomes unimportant.

Natural selection, however, always remains the court of last resort, and natural selection always determines whether a new form

will persist or become extinct. Chance mutations, the sampling error, and natural selection, taken together, can account for all the known facts of evolution.

The theory of evolution is actually the central problem of the whole of biology. It has passed through all the developmental stages of philosophy, of observation in nature, and of experiment in both botanic gardens and in zoological laboratories. Systematics, palaeontology, anatomy, embryology, cytology, and genetics all combine in the study of evolution. Evolution constitutes one of the most comprehensive branches of biology, covering as it does, microbes, plants, animals, and human beings.

12

Broad Daylight

With this chapter we bring our history of biology to a close; but, like all such histories, it has to end without the drama of a climax. In fact, no history of science ever ends with a bang, or even with a whimper. The historians merely stop writing. Just where they stop, perhaps, is not too important. Always it has to be at some arbitrary point, because science itself never stops. Today, science continues to speed ahead, just as it has been speeding for the past five centuries, and the rate of increase of its progress is still exponential. As long as science grows at such a rate, we can be sure that it is still in its youth and that it has traveled only a little way along the path that some day will mark its entire course. We realize that its exponential growth rate cannot last forever, because nothing can grow forever at such a rate—at least on a planet of finite size. As we know, all growth curves become sigmoid as they mature, and this means that the growth curve of science will have to flex and that its sigmoid shape will ultimately become evident.

Perhaps in some future age the historians of science will be able to tell the whole story of science but, today, we face no such sad eventuality. Like historians in other fields, we simply end our history at a point that suits our convenience. But in doing so our action is not wholly capricious. Neither is it wholly satisfactory. Ideally, the history of a science should fuse into and connect with the current textbooks; but today our texts do not remain current long. Thus there has to be a hiatus between what the history covers and the discoveries that are being made in the ever moving present. But

the hiatus need be neither stationary nor permanent. It should travel along behind the advancing front of science somewhat like the trough behind a moving wave. It should advance with every rewriting of the history, just as science itself advances.

The gap between the scientific discoveries and the latest history of science need be neither wide nor deep, and certainly it need never be uncrossable. In fact, it has already been bridged with a group of special periodicals—the review journals. These journals were established originally to make it possible for specialists to keep in touch with what other specialists were doing, and to bring some order and system into the sciences that were growing so fast they were leaving their acolytes behind. These review journals are also a very effective aid to historians of science. They are numerous, well edited and, in general, well written. We can list only a few of them here.

The science of genetics, for example, is served by *Bibliographia Genetica, Advances in Genetics,* etc. Other biological sciences are aided by *Biological Reviews, Botanical Reviews, Physiological Reviews, The Quarterly Review of Biology,* and numerous others. These and other review journals are now supplemented by the publication of numerous *Symposia* and *Continuations* and by the oddly-designated *Handbücher* which are generally published in many weighty volumes that are much too heavy to be held in the hand. All of these publications help preserve some system in the biological sciences. Without them, the thousands of periodicals that print some 125,000 individual contributions a year would doubtless confuse and bewilder the biologists with more material than they could possibly assimilate.

In this final chapter, we can do little more than list a few of the more startling advances in biology that have been made during the past few years, and connect them with the background discoveries that made them possible. We shall also attempt to view the biology of today from the perspective of its immediate past.

It is only stating a truism to say that the progress of biology has always been conditioned by the physical and intellectual equipment of the biologists. Most of their physical equipment they created for themselves, but sometimes they got their more sophisticated apparatus and techniques from discoveries made in other sciences. Today, the biologists are deeply indebted to both the physicists and the chemists. They are indebted to the physical scientists both for

the electron microscope and for the many radioactive isotopes that have been so useful in the investigation of cellular physiology. In the absence of such research tools as these, biology today might be but a little ahead of where it was a generation ago.

The more recent biological advances fall easily and naturally into two groups, or classes. In one group belong the discoveries that were made through the application of research techniques that have been in existence for many years. These were the discoveries that would have been made routinely in any healthy, growing science. The discoveries that belong in the other group, however, are those that could have been made only by the use of instruments that have only recently been created. These are the discoveries that no past generation of biologists could possibly have made. However, the two types of discoveries supplement each other admirably and, together, they have revolutionized biology.

In 1895, W. Röntgen announced his discovery of x rays. During the next quarter century, x-ray photographs were used by physicians as a most valuable diagnostic tool. In fact, x rays were so widely used that it became only a matter of routine precaution to test them for any deleterious effects they might have on the patient. A number of contemporary biologists investigated their effects on living tissue and soon recognized and described the destructive consequences of excessive radiation. Their publications were numerous and their work was important and should not be overlooked. Among the first to record the effects of radiation on living tissues were Schober (1896), Seckt (1902), Koernicke (1904), and Gager (1908). This was of course long before the atom bomb had made our contemporaries radiation-conscious. In 1927, H. J. Muller treated the famous fruit fly Drosophila with x rays and was able to prove, by means of some cleverly designed experiments, that the x rays had caused a number of the Drosophila genes to mutate (*Science* 66:84–87). These were the first mutations to be induced artificially.

The next year, L. J. Stadler showed that x rays could also cause mutations in plants (*Science* 68:186). The way was now clear for a new type of experimental genetics, since a great many organisms whose heredity was calling for investigation could not be studied effectively because they lacked an adequate store of mutations whose transmission from generation to generation could be followed in detail. But following Muller's discovery, there need be no short-

age of mutations. Artificially induced mutations were now available and they were soon developed into a powerful tool for research. In 1946, C. Auerbach and J. M. Robson (*Nature* **157**:302) discovered the mutogenic effects of mustard gas, thus showing that mutations could also be produced by chemicals. At present, a great many mutogens are known, and artificial mutations can be produced almost at will. But it is important to remember that desirable mutations still cannot be made to order.

We cannot include here the many discoveries in genetics that resulted from the use of artificial mutations. One discovery, however, is outstanding, not so much as a contribution to genetics per se, but through the fact that it created a unique tool for genetical and for biochemical research. In 1941, G. W. Beadle and E. L. Tatum (*Proc. Nat. Acad. Sci.* **27**:499–506) described their work on induced mutations in the ascomycete, *Neurospora crassa,* a fungus that is now the subject of a widespread and intensive investigation. Beadle and Tatum were able to produce mutant strains that would grow on their synthetic media only when one or another amino acid or vitamin had been added. In this way, they could identify the biochemical deficiencies of the mutations that they had induced, and this put them in a position where they could relate the mutant gene to the physiological processes that took place within the living cell.

It was now possible to block one or another of the biochemical reactions genetically without treating the cell with reagents that might have unidentified side effects. The one-to-one relationship of a gene to a specific chemical end-product had been known, of course, for a long time—ever since Mendelism was found to apply to the inheritance of the anthocyanin pigments in flowers. Beadle and Tatum, however attacked the problem from the other end and identified chemical reactions that seemed to be much closer to the genes.

The composition and structure of the genes themselves have at last become the subject of chemical analysis. The first step was taken by F. Miescher in 1871, when he isolated the nuclei from the cells of pus and found that they contained a substance, later called "nucleic acid," that was soluble in alkalis but insoluble in acids. In 1924, R. Feulgen and H. Rossenbeck, and R. Feulgen and K. Voit devised a color test that was specific within the cell for their "thymonucleic acid," and their test showed that the acid occurred only in the chromosomes. Their testing procedure was soon named the "Feulgen reaction."

This localization of what we now call "deoxyribonucleic acid" (DNA) was the first real step toward the chemical analysis of the chromosomes. It initiated an intensive investigation into the chemistry of chromosomes—into their DNA and their amino acid content. This led to a major problem in structural chemistry. As this is written, the spectacular Watson-Crick diagram of the structure of the DNA molecule (Figure 60) together with its attachments, gives us

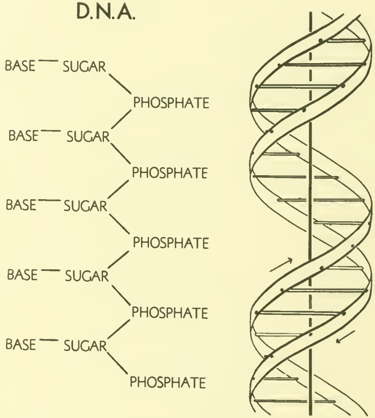


Figure 60. Diagram of structure of DNA in a chromosome. (From Watson and Crick, 1953.)

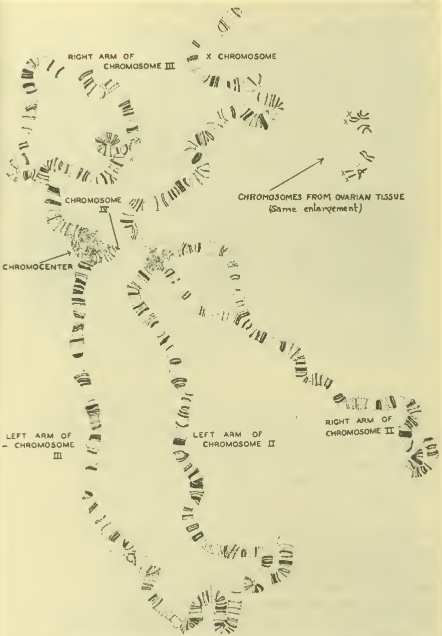
our present working model of the structure and the chemical composition of the material of heredity. (*Cold Spring Harbor Symposia* 18:123–131 [1953]).

Meanwhile, the genetic and cytological analysis of the chromosomes had made it possible to locate the genes with great accuracy. Morgan, Sturtevant, Bridges *et al.*, had shown by their studies of linkage and crossing-over, that it was possible to diagram each linkage group or gene-string as a straight line with each gene occupying its unique locus or position in the line. Such a diagram, drawn to fit the cross-over ratios, was simple and self-consistent, and showed very precisely the linear order of the genes. Somewhat prematurely, the diagrams were called "chromosome maps," although they were only graphs of arithmetical ratios. The consistency of the ratios, however, indicated that they must have a physical basis and that the basis was in the chromosomes.

In 1929, T. S. Painter and H. J. Muller (*Jour. Heredity* **20**:287–298), and Th. Dobzhansky (*Biol. Zentralb.* **49**:408–419), correlated some of the alterations and fragmentations they had observed in the chromosomes of Drosophila—such structural changes as translocations and deletions—with changes in the loci of certain genes. It thus became possible for them to place known genes in different portions of the chromosomes—place them in particular regions of the material bodies that could be seen under the microscope.

Unfortunately, the chromosomes in the germ track of Drosophila were extremely small, and the location of the genes had to be far from precise. But in 1934, Painter (*Jour. Heredity* **25**:465–476) described and pictured the chromosomes in the salivary gland of Drosophila, and this disability was removed. These salivary gland chromosomes were a hundred times as long (and broad in proportion to their length) as those in the germ cells. Moreover, they were marked throughout their entire length with distinct and easily recognizable disks or bands. Figure 61 shows the relative size of the germ cell chromosomes compared with those in the salivary gland. It also shows the bands on the salivary gland chromosomes.

The discovery of these giant chromosomes made an accurate and detailed mapping of the Drosophila chromosomes possible. The maps were first drawn by Painter (*op. cit.*) and by C. B. Bridges (*Jour. Heredity* **26**:60–64 [1935]; **29**:11–13 [1938]). Known genes could now be located in single recognizable bands, and the structural alterations that sometimes took place in the chromosomes themselves—the deletions, inversions, translocations, reduplications, etc. —could be mapped precisely by recording which of the marking bands had changed their "normal" positions. Now, at last, some



GIANT CHROMOSOMES COMPARED WITH "NORMAL"

Figure 61. Salivary gland chromosomes of Drosophila compared with chromosomes in the germ track. (From Painter, 1934.)

of the intra-species evolutionary changes could be correlated with known chromosomal alterations. Here, serendipity was working overtime. The importance to modern genetics of the discovery of the salivary gland chromosomes can hardly be exaggerated. But these odd nuclei in the salivary glands of the Diptera had been known for half a century when Painter first interpreted their structure correctly.

In 1881, E. G. Balbiani described and pictured these striking nuclei in the salivary glands of the larva of *Chironomus plumosus* (Figure 62) and a number of cytologists followed him in describing

Figure 62. Nuclei in salivary gland cells of *Chironomus plumosus.* (From Balbiani, 1881.)

their structure in considerable detail. Here we need mention only the work of Fr. Leydig (1883), E. Korschelt (1884), H. Bolsius (1910), H. Erhard (1910), and M. A. Van Herwerden (1910). F. Alverdes (1912) traced the growth and development of these peculiar nuclei and his excellent illustrations (Figure 63) are still worth our attention. W. Faussek (1913) also described their development and E. Tanzer (1921) reported that they occurred only in Diptera. D. Kostoff (1930) once again called attention to their discord structure but B. P. Kaufman (1931) held that this structure was due to a faulty fixation. The year before Painter's work appeared, E. Heitz and H. Baur reported that the chromosomes in the salivary gland cells of *Bibio hortulorum* were double chromosomes and that the number of pairs was half that of the number of chromosomes in the other somatic cells. With Painter's paper of 1934, however, the salivary gland chromosomes were finally brought into the field of genetics.

Other biological sciences have advanced just as rapidly as genetics and, in such fields as medicine and public health, they have had a

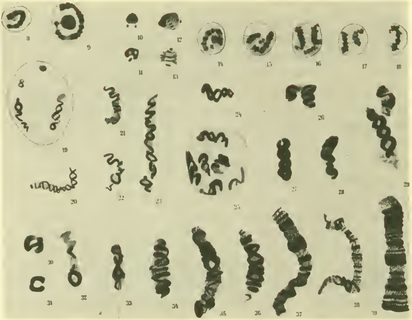


Figure 63. Development of salivary gland chromosome of *Chironomus plumosus*. (From Alverdes, 1912.)

more immediate application. We need mention only such familiar terms as "vitamin" and "antibiotic" to emphasize the extent to which our personal well being depends upon the more recent biological discoveries. In this chapter however, we shall have to treat all recent advances rather cavalierly, and limit our account to tracing their origin within the background of our general biological information.

Today, the public takes vitamins for granted; but they have been in general use only about thirty years. Before the present generation, our species suffered from vitamin deficiency chronically and at times acutely. Ever since our ancestors learned how to grow crops and to cook their food, their diets have been in some respects defective. Before they were civilized, however, they were often hungry enough to eat anything they could find that was edible. With the advent of agriculture, however, they ate more; but they also ate fewer kinds of food. Undoubtedly they showed many of the symptoms of malnutrition—symptoms that we would recognize today. At

the time, however, such symptoms were so common that they were looked upon as the normal afflictions of the human condition.

As we would expect, diseases due to dietary deficiencies would attract more attention in regions where they were most acute and where the diet was least satisfactory. Perhaps no human diet was ever worse than that of medieval Europe, especially the diet in the northern and western nations. There, during this period, such diseases as scurvy and rickets were recognized and described. Effective herbal remedies were even prescribed, for instance, in the Netherlands where "lepelbladen" (*Cochlearia officinalis*), or "scurvy grass," was an often-used household remedy.

"Sea-scurvy," an acute form of the disease, became a major hazard when long sea voyages were first undertaken and the sailors ate little else but salted meat. Feeding the sailors so that they would not fall to pieces and die was a real problem, and the longer the voyages the more important it was to add fresh vegetables to their diet. Indeed, South Africa was first settled by the Dutch to supply fresh vegetables to their sailors, who had to make the long trip around the tip of Africa to the East Indies. In 1652, the famous garden at the Cape of Good Hope was planted, and a fort built to protect the gardeners. This was the beginning of the Cape Town settlement.

The vitamin story proper can be traced back to the eighteenth century, when it was discovered that scorbutic sailors could be cured by certain specifics. In 1734, Johanne Bachstrom (*Observationes circa Scorbutum*, Leiden) narrowed down the list of foods that would cure scurvy and coined the word "antiscorbutic." Toward the end of the century, the British Navy prevented the disease when they added lime and lemon juice to the food of their seafaring men. Other deficiency diseases persisted, however, and rickets was still known as the "English disease."

In 1897, C. Eijkman (*Virchow Archiv.* 149:187–194) proved that beriberi was not caused by a germ but arose through a dietary deficiency associated with a monotonous feeding habit based upon polished rice. A. C. Pekelharing, who had been Eijkman's teacher, saw clearly that in addition to the fats, carbohydrates, and proteins there was a whole class of foods which, in minute quantities, were necessary to maintain life. He stated this clearly and forcibly in 1905 (*Nederlandsch Tydschrift v. Geneeskunde,* 51). In 1917, E. V. McCallum and N. Simmons showed that Xeroma was caused

by the lack of a fat-soluble substance they called *Vitamin A.* In 1919, J. C. Drummond named the anti-scorbutic preventative *Vitamin C*, and, in 1922, McCallum reported that rickets was produced by the lack of a food factor which he called *Vitamin D. Thiamin* (*Vitamin B*) was crystallized by B. C. F. Jansen and W. F. Donath in 1926, and H. M. Evans identified *Vitamin E* in 1922.

The first practical antibiotic, *penicillin,* was extracted and its properties recognized by Alexander Fleming in 1929 (*Brit. Jour. Ex. Path.* **10**:226–236). Its source was the common green mold, *Penicillium notatum,* a common contaminant of most biological laboratories. Actually, the fact that colonies of many bacteria would not grow on a plate close to a *Penicillium* contaminant had been observed by nearly every bacteriologist whose technique was not good enough to prevent the contamination. But only a few of them recorded this fact. In 1876, however, John Tyndall not only described the antibiotic properties of *Penicillium* but showed that he had grasped completely its biological significance. In his experiments on air-born infection, he exposed a number of tubes containing nutrient solutions to the air. He reported (*Nature* **13**:269):

> The singling out, moreover, of one tube of the hundred by the particular *Bacteria* that developed a green pigment, shows that, as regards *quality,* the distribution of germs in the air is not uniform. The same absence of uniformity was manifest in the struggle for existence between the *Bacteria* and the *Penicillium.* In some tubes the former was triumphant, in other tubes of the same infusion the latter was triumphant.

It is not practical to list here all of the antibiotic discoveries made between the contributions of Tyndall and Fleming. We will cite, however, several of those in which *Penicillium* itself was involved. In 1896, B. Gosio (*Riv. Iziene Sanit. Pub.* **7**:825–849, 961–981) extracted a crystalline substance from *Penicillium* that inhibited the growth of the *Anthrax* bacillus, and the next year, E. Duchesne, in his dissertation on the antagonism between the molds and the microbes (Lyon, 1897), stated that "certain green *Penicillia* are capable of suppressing the growth of various bacteria or of bringing about their attenuation." In 1908, A. Sturli (*Wien. Klin. Wschr.* **21**:711–714) extracted a poison from *Penicillium glaucum* that stopped the growth of the *Anthrax* bacillus. From 1923 to 1939, A. Gratia and his co-workers published a number of short papers on the antibiotic properties of the actinomycete. In 1925 (*C. R. d. Soc. Biol. d. Paris* **92**:461–462), they extracted from *Penicillium glaucum* a substance

that would dissolve the *Anthrax bacillus*. But before Fleming's classical paper the biologists in general had not been able to purify the antibiotics sufficiently to make them practical therapeutic agents—although at times one or another of them were reported to cure diverse infections. It is worth emphasizing that penicillin was *not* the first antibiotic used in treating human patients; but the antibiotics used earlier were accompanied by the living bacteria or fungus that produced them.

Perhaps the majority of soil micro-organisms produce antibiotics that inhibit the growth of their competitors and, if produced in sufficient quantities, ultimately destroy them. Up to 50 per cent of the actinomycetes have been found to produce antibiotics and we can easily see how this property of theirs has been selected by nature. In the soil, the struggle for existence takes the form of chemical warfare and our modern physicians have learned—very fortunately for us—to use some of this chemical ammunition. Many a life has been saved by an antibiotic that was more toxic to our pathogenic invaders than it was to us in our role as hosts. But most antibiotics are just as toxic to those who are invaded as they are to the invader. In 1940, S. A. Waksman and his co-workers discovered "actinomycin," which turned out to be useless because it is one of the most poisonous substances ever produced by a living organism. In 1944, however, they found the useful "streptomycin" (*Proc. Soc. Exptl. Biol. Med.* **55**:66). Today, we have a host of antibiotics to choose from.

As we have shown above, the discovery of antibiotics preceded their therapeutic use by more than half a century. Those who made the discoveries were often working on different problems and had different objectives in mind. Tyndall, for example, discovered the antagonism between *Penicillium* and certain bacteria in experiments designed to test the current hypotheses of abiogenesis. He found that, on the bacterial level, life did not originate *de novo*. But the great question as to the origin of life (*see* p. 274) is still with us, and biologists with imagination are still attempting to solve it. Today, however, the problem is on a much more sophisticated level and the steps leading from the non-living to the living are now being surveyed one at a time. Spectacular progress has been made in exploring the structure of the DNA molecule—for here, seemingly, life started. Great progress is also being made in the investigation of the connections of DNA with its accompanying amino acids. The

biochemists have already reached a point where they have to be more than a little arbitrary when they distinguish between the "living" and the "non-living." The phase of their research that concerns us here, however, is the one that attempts to show how life could have originated on earth in the absence of biochemists.

Here we have to assume that "nature in her nakedness," with her innumerable puddles, pools, wet-weather lakes, and tidal flats, with her volcanic ovens and retorts, with her lightning that could strike almost anywhere, and above all, with billions of years at her disposal, could, after her own clumsy fashion, accomplish whatever man can—although we know that nature can never be quite as elegant as our own better biochemists. During the past two decades, numerous books and papers have been published on the origin of life, perhaps the most influential—certainly the most talked about—have been A. Oparin's *Origin of Life on Earth* (1961) and H. C. Urey's *The Planets, Their Origin and Development* (1952). The present consensus of opinion is that life originated where there was no oxidizing atmosphere, and under conditions where molecular hydrogen was present and where carbon and nitrogen existed as reduced compounds (Wherry, 1936). Under laboratory conditions similar to these surroundings, amino acids have actually been produced.

In 1953, S. L. Miller (*Science* **117**:528–529) filled a container with an atmosphere composed of hydrogen (10 cm. pressure), methane (20 cm. pressure), and ammonia (20 cm. pressure). Boiling water was added and the whole was subjected to electric discharges. At the conclusion of the experiment, α alanine, β alanine, glycine, and aspartic acid had been concocted, as were a number of other, unidentified compounds. At last, we seem to be getting a picture of the surroundings in which the building blocks of the complex compounds that we now consider "living" could have been formed.

Such an experiment as Miller's could have been done much earlier if anyone had been bright enough to have thought of it. Many of the recent advances in biology, however, had to await the invention of new implements. The greatest addition to biological implementation in the past half century has been the *electron microscope.* The light microscope cannot magnify beyond fifteen hundred diameters without losing resolution. Microscopes, adapted for the ultraviolet, can preserve their resolution at a magnification twice as great. The recently devised *phase-contrast* microscope enabled cytologists

to see details in living, unfixed, and unstained preparations that hitherto had been obscure. But the electron microscope extended the range of effective magnification two whole orders of magnitude, and even at a hundred-thousand diameters the resolution it gave was clear and sharp. For the first time virus particles could be seen, and virology assumed the status of an independent science. Some of the different strains of bacteriophages can now be identified optically, and the structure of viruses has become something more than a chemical problem. The electron microscope, of course, has its limitations, but to modern molecular biology it has proven invaluable.

In 1926, H. Busch (*Ann. Physik* 81:974) demonstrated that a properly-shaped magnetic field could refract a beam of electrons much as a lens could refract a beam of light, and in 1931, E. Ruska and M. Knoll (*Zeit. Tech. Physik.* **12**:389) utilized this property of the electron beam to construct an electron microscope. They were able to take photographs at a very high magnification. Three years later, L. Marton (*Ann. Bull. Soc. Roy.:Sci. Med.–Nat. Bruxelles* **92**:106) applied the new instrument to biological material, and photographed bacteria. In 1938, F. Krause (*Naturwissenschaften* **26**:122) made electron photographs of virus particles, and from this date on, the electron microscope became an essential tool for certain types of biological research, although naturally the electron microscope was not limited to biology. It was used, literally, to examine everything that was small enough to place in its specimen chamber, and the enthusiasm of the electron microscopists was as great as that of the light microscopists three centuries earlier.

Newer cytological techniques were needed, of course, as living material was unsuited for electron examination. Fixation by quick freezing, followed by a dehydration in which the frozen water was removed by deliquescence in a vacuum, preserved the cell organs with less distention than they had when they were fixed with the older chemical methods. Newer microtomes, with spinning blades, were devised that could cut sections so thin that details hitherto invisible could be seen even when the specimen had been fixed with osmium tetroxide. And even stationary microtome knives have now cut sections half a micron thick. Needless to say, the newer techniques have added greatly to our knowledge of the morphology of the cellular organs.

It is now possible to extend genetic investigation to both the

bacteria and the viruses. In 1946, J. Lederberg and E. L. Tatum (*Nature* 158:558), and the next year Lederberg (*Genetics* 32:505–525), reported that two strains of *Escherichia coli,* each of which had deficiency mutations, could combine to form *prototrophs,* which showed none of the deficiencies. The hybrid bacilli could form colonies on media that would support neither of the parental strains. Although many bacteria seem to have lost the ability to combine sexually, Lederberg and Tatum have proven that the common colon bacillus still retains some of its sexual potency and habit.

Sexual reproduction has even been reported in the viruses. In 1949, S. E. Luria and R. Dulbecco (*Genetics* 34:93–125) found that when two or more particles of a bacteriophage that had been inactivated with ultraviolet light penetrated a single bacterium, the bacterium, on lysis, would release the normal infective phage. In the same year, H. D. Hershey and B. Rotman (*Genetics* 34:44–71) proved that a recombination of genetic factors occurs when diverse phage particles enter a single bacterium. Two particles that differed from each other by four such factors produced, when the phage emerged on lysis, particles in which the factors appeared in four different combinations. Three years later, N. D. Zinder and J. Lederberg (*Jour. Bact.* 64:679–699) discovered that sex in the microscopic world could even be achieved by proxy. They discovered that phage particles could take specific genetic factors from one strain of bacteria and incorporate them in another. This process is known as *transduction.*

Viruses, as we have stated, are unable to lead an independent life; they can reproduce only within the living cells of the higher animals and plants. The obvious corollary is that viruses may have originated spontaneously, but that they could never have lived and reproduced in the absence of the more highly evolved forms. Obviously the higher animals and plants would have had to have preceded at least those viruses that we are familiar with. (Of course, non-parasitic, non-saprophytic, and purely chemophagic viruses would be very difficult to discover, even if they still exist.) Yet, in the chemical investigations of viruses, we have approached closer to the origin of life than we have in any other line of biological research. In 1955, H. Fraenkel-Conrat and R. C. Williams (*Proc. Nat. Acad. Sci.* 41:690–698) separated the tobacco mosaic virus into its protein and ribose nucleic acid components. When separated, these fractions could not reproduce or infect the tobacco

plant; but, when they were combined, they could. Here a living virus, if we consider viruses as living, was synthesized from its nonliving building blocks. Whether we consider this to be a "creation" of life is purely a matter of how we define life.

Next to the electron microscope, perhaps the greatest recent gift to the biologists has been an abundance of radioactive isotopes. With these they have been able to label a number of atoms and follow them through a number of metabolic pathways. Cellular physiology has naturally made great progress. Such isotopes as P^{35} and I^{131} can accumulate in particular regions to an extent that their presence can be demonstrated by auto-radiographs.

The use of non-radioactive isotopes has also led to major discoveries and, incidentally, to correcting some long-standing errors. To cite one example, a very logical assumption concerning the chemistry of photosynthesis has been shown to be false. In the processes involved in photosynthesis, nature apparently had set a very clever trap—a snare that held the physiologists from the time of N. T. de Saussure (1804) until the trap was broken in 1941.

De Saussure had reported that carbon dioxide was absorbed in photosynthesis and that the oxygen it contained was released. The fact that the quantity of the released oxygen was equal to that absorbed in the carbon dioxide was, of course, good presumptive evidence that the dioxide was the source of the oxygen, an assumption that led to the invention of the "photosynthetic quotient." Anyone who suggested that the oxygen might have come from water, was, of course, only being difficult. Today, the problem stands on a very different footing.

In 1939, R. Hill (*Proc. Roy. Soc. London B.* **127**:192–201) obtained a continuous production of oxygen from illuminated chloroplasts in the presence of a hydrogen acceptor. Ten years earlier, C. B. van Niel (*Contributions to Marine Biology*, pp. 161–169, Stanford, California: Stanford University Press) had analyzed the steps that occurred in the photosynthesis of the purple sulphur bacteria and he had suggested that here the water acted as the hydrogen donor and that, as a corollary, the oxygen released in the reaction must come from water and not from the carbon dioxide. This idea of van Niel's turned out to be applicable to photosynthesis in general as well as to his bacterial photosynthesis. It was verified in 1941 by S. Rubin, M. Randall, M. Kamen, and J. L. Hyde (*Jour. Am. Chem. Soc.* **63**:877–879) when they used oxygen eight-

een (O^{18}) as a tracer. First, they introduced the O^{18} into the carbon dioxide and found that the oxygen liberated in photosynthesis was the usual O^{16}, and thus did not come from the carbon dioxide. When, however, they incorporated the O^{18} in the water, it was released as the by-product of photosynthesis.

Our precise knowledge of the respiratory enzymes is also very recent. It has come from discoveries that are strictly of the twentieth century, although the essential clue that led to the discoveries can be found well back in the nineteenth. As early as 1837, J. J. Berzelius had stated that fermentation was achieved through a catalyzed reaction and, in 1839, J. Liebig claimed that non-living systems were able to carry on fermentation. Twenty years later, however, Louis Pasteur stated that living bacteria brought about lactic acid fermentation and, in 1871, that living yeast cells were necessary for the fermentation that produced alcohol. But here Pasteur was mistaken. In 1897, E. Buchner (*Ber. deuts. Chem. Gesell.* **30**:117–124) proved that living yeast cells were not necessary for alcoholic fermentation. He was able to produce alcohol from the fermentations of glucose, fructose, and maltose by an extract that he had pressed out of ground and crushed yeast cells. This discovery of Buchner's removed the "vital" element from enzyme action.

The history of respiratory enzymes also shows a series of fluctuating beliefs. This history, perhaps, is more ancient than honorable. In 1886, C. A. MacMunn (*Phil. Trans. Roy. Soc. London* **177**:267–298), by a skillful application of microspectroscopy, identified a group of pigments he labeled "myohaematin." His conclusion is still worth quoting. In regard to these pigments he said:

> Their bands are intensified . . . by reducing agents and enfeebled by oxidizing agents; they accordingly appear to be capable of oxidation and reduction and are therefore respiratory. . . . They combine with the oxygen conveyed to them in the blood, and hold it for the purpose of metabolism, parting with the carbon dioxide in exchange for the oxygen. . . . These observations appear to me to point out the fact that the formation of carbon dioxide and the absorption of oxygen take place in the tissues themselves and not in the blood.

In 1889, however, L. Levy (*Zeits. f. Physiol. Chem.* **13**:309–325) claimed that MacMunn's respiratory enzymes were merely pigments derived from haemoglobin. MacMunn replied to Levy's criticism in the next volume of the *Journal* (14:328–329), but the very influential editor, F. Hoppe-Seyler, appended an editorial note

to MacMunn's reply, stating that MacMunn had added no new data and that he, F. Hoppe-Seyler, considered all further discussion to be superfluous. This authoritarian ruling disposed of MacMunn and settled the question. The debate ended and the investigation of the role of MacMunn's pigments, which we now call the "cytochromes," was dropped for thirty-five years. Hoppe-Seyler's successful assumption of the role of Deity was more unfortunate.

In 1925, D. Keilin (*Proc. Roy. Soc. London B.* 98:312–329) rehabilitated MacMunn and proved that MacMunn's pigments, which he renamed the "cytochromes," were found in all aerobic organisms. He also showed that their function was respiratory. By 1930, Keilin had identified three different cytochromes. We now know that there are some twenty-five! Research in the field of these respiratory enzymes has become exceptionally active, and factual knowledge is accumulating rapidly. There have been, of course, a number of controversies, which naturally is to be expected in any rapidly growing and important branch of science. Keilin may be said to have established a school, and he has found a very able rival in Otto Warburg, from whose laboratory a number of basic discoveries have also come. Here, however, we are not concerned with the relative value of the rival contributions. Physiology is heavily indebted to both groups and to the subsequent investigations that are now taking place in most of the physiological laboratories of the world.

One very elegant contribution should be mentioned. In 1937, H. A. Krebs and W. A. Johnston (*Enzymologia* 4:148–156) described a respiratory pathway now called the "Krebs Cycle." In this cycle six different stages in the oxidation of pyruvate succeed one another in regular order, the intermediate reagents are regenerated successively, and one revolution of the cycle results in the complete oxidation of pyruvate.

There is no doubt that the real value of the great advances made in biology during the twentieth century lies in the contributions they have made to our intellectual life—in the addition they have made to our knowledge and to our understanding of the great organic world of which we are a part. These advances also have added appreciably to our physical well-being. Our recently acquired knowledge of vitamins, for example, enables us to avoid the disabilities of bad diets; our knowledge of antibiotics enables us to recover from many infections that otherwise would kill us. It is

true that many of the practical applications of biology, such as those that satisfy our dietary needs and preserve our health, are not new. From the earliest times biology has had a practical value. The major discoveries in agriculture, for example, were made before history started; and even in the field of agriculture, biology has continued to make its contributions. Perhaps the most spectacular of these has been the development of hybrid-corn.

In 1920, seed from the first hybrid corn was put on the market in Connecticut following the work of D. F. Jones, and, the next year, seed from another cross was sold to the farmers of Iowa. This marked the beginning of the commercial exploitation of *heterosis*, or *hybrid vigor*. The scientific basis for this vigor, however, had been discovered much earlier. The fact that hybrids were exceptionally vigorous has been known from the earliest times, but its explanation had to await the twentieth century.

When Indian corn (*Zea mays*) is self-pollinated, its progeny is smaller, more fragile, yields less, and is, in every way, inferior. Continuing the selfing results in a continuing loss of vigor for from some six to seven generations, at which point the corn is practically homozygous and, from then on, it experiences no further decline. When two such selected, inbred strains are crossed, their hybrid progeny regain not only the original vigor of their parental stocks but show, in addition, a marked improvement over their open-pollinated ancestors. Four inbred stocks can be combined by crossing two different first generation hybrids. If the four stocks have been selected wisely, the second generation hybrids will give an increased yield of from twenty-five to thirty per cent. When such carefully pedigreed hybrids are given the best possible cultivation and all of the fertilizer they can use, the yield can be doubled. The discovery of hybrid corn, literally, has doubled the production of food in some of the lands where it can be grown. By 1940, hybrid corn had displaced the open-pollinated varieties in the corn belt of the United States.

Hybrid corn represents a modern and very skillful application of a very ancient principle of breeding. Some of the advantages of cross-breeding were doubtless recognized in prehistoric times as is shown by the traditional laws against incest and by the widespread custom of not marrying a near relative. But in this custom there was naturally some confusion. The custom was not consistent, as it decreed also that the Pharaohs and the Incas should marry their

sisters. Shepherds and herdsmen, however, were well aware of the advantages of cross-breeding; but in the vegetable kingdom, cross-breeding had to await the discovery of sex in plants.

As we have noted earlier, Koelreuter (1761–1766) recorded the greater size of many of his plant hybrids, and Sprengel (1793) described some of the floral mechanisms that enabled plants to secure cross-pollination. Knight (1799) noted the greater size of his hybrid plants, and this increase in size was recorded routinely by nearly all the plant breeders of the nineteenth century. In 1876, Darwin published *The Effects of Cross and Self Fertilization in the Vegetable Kingdom*, and here he discussed the matter in detail. In his classic paper, Mendel (1865) recorded an instance of hybrid vigor, but he recorded it only incidentally.

The breeding experiments that led to the development of hybrid corn were carried on chiefly in our state experiment stations. In 1880, W. J. Beal described how he had increased the yield fifty per cent by planting two different varieties of corn in alternate rows. During the following decade, W. A. Kellerman, W. T. Swingle, and W. M. Hays (*cit.* Singleton, 1935) reported dominance, recessiveness, and even Mendelian ratios, but apparently without realizing their significance. J. W. Sanborn (1890) found that the hybrid gains were lost in part when the hybrid stock was reproduced by open-pollination. S. W. Johnson (1891) explained the vigor by assuming that each of the parental stocks covered up the weaknesses of the other.

In 1892, G. W. McClure published a short paper in which he showed that: (1) sterility and deformity often follow selfing; (2) that crossing imparts vigor; (3) that it is impossible to tell in advance what varieties will produce corn of an increased size when crossed; (4) that what appears to be the best ear does not always produce the best crop; and (5) nearly all the hybrid corn that is grown the second year is smaller than that grown the first year. In 1900, H. J. Webber found that, when he crossed a Peruvian corn with a variety from the United States, the hybrid was over twelve feet tall, although both parental stocks were only a little over eight feet in height. (Figure 64)

The real understanding and genetic explanation of hybrid vigor were obtained during the short period from 1906 to 1908 by G. H. Shull and E. M. East working independently. Shull's classic paper, "The composition of a field of maize" (1908) and East's "Inbreed-

Figure 64. Hybrid vigor shown in a cross between a Peruvian and a North American corn. (From Webber, 1900.)

ing in corn" (1908) placed the problem in its proper Mendelian setting. When East's student, D. F. Jones (1917, 1918) refined the current interpretation and showed the advantages of combining four inbred strains, the scientific background of *heterosis* was established.

The preliminary inbreeding that made the parental stocks homozygous was essential in that it brought many of the recessive defects to the surface, where the worst ones—the lethals, semilethals, sterility genes, etc.—were eliminated automatically. The hybridization that followed covered up the recessive genes that remained in the stock. As most recessive genes are deleterious when

homozygous, covering them with their dominant alleles removes their weakening effects. Even dominant genes may be defective in some ways, but these defects can also be rendered harmless if the stock is heterozygous—i.e., if the two dominant genes at the locus are different and have different disabilities. Under such conditions each gene may correct the defects of the other. A small book, *Inbreeding and Outbreeding: Their Genetic and Sociological Significance,* published by East and Jones in 1919 summarized the work on hybrid vigor and placed the principles of heterosis at the disposal of the practical breeders.

We have now reached a point where we can bring our history of biology to a close. The ending, however, is only the ending of an installment, because the story itself never ends. Biology is still making history; but, in all due deference to the science, it does sometimes seem to be a little like Stephen Leacock's hero who "mounted his horse and rode off in all directions." Biology *is* branching out into fields where it has never penetrated before. Of course, all of the branches will not develop into main trunks; many of them will remain little more than twigs. And some of the twigs will drop off and be forgotten. No one, however, can look at a new growth and foretell its future. In the past, many popular trends have ended in an *impasse,* and minor, overlooked, and even casual discoveries have been the precursors of major advances. This, however, is not an argument to persuade biologists to observe and experiment at random. We have to plan our research, of course, but not all of our plans can be realized. Sometimes the biologist that history remembers was one who was alert enough to take advantage of his luck and who recorded some unexpected observation. Serendipity has always been a factor in the progress of science.

Sometimes the fortunate discovery is made in a very distant field and, when this occurs, it shows how valuable a bit of intellectual cross-fertilization can be. This is illustrated beautifully by the development of radiocarbon dating.

When cosmic rays strike the earth's atmosphere they produce neutrons which have a half-life of thirteen minutes. These neutrons never reach the earth's surface, but react with the atmospheric nitrogen to produce hydrogen and *carbon fourteen* (C^{14}). The hydrogen does not concern us here but the carbon fourteen does. The carbon fourteen has a half-life of $5{,}568 \pm 30$ years. First, it is burned by the oxygen of the air to form carbon dioxide, which be-

comes mixed, of course, with the usual carbon dioxide that contains C^{12}. Inasmuch as C^{14} is being formed and destroyed at a constant rate, its proportion to C^{12} in the air remains constant. Through the process of photosynthesis, C^{14} becomes incorporated into living plants at a constant rate and, as the ultimate food of animals consists of plants, it is also being incorporated into animals—also at a constant rate. In the animals and plants, it of course decays, and the decay continues after the animals and the plants die—but after they die, they can no longer assimilate C^{14}. Thus all organic debris —all bones, leaves, twigs, and trunks—lose their C^{14} at a known rate. Thus, by measuring the amount of C^{14} in any compound of organic origin, the age of the compound can be determined. For example, if a bone needle has just half the C^{14} that a modern bone would have, it would have been made from an animal who died $5{,}568 \pm 30$ years ago.

The technique of dating organic remains now in general use in archaeology and anthropology was devised and refined by W. F. Libby, and by his students, and co-workers (W. F. Libby, *Radiocarbon Dating*, Chicago: University of Chicago Press, 1952). Today, we can date with fair accuracy an Egyptian mummy, a bit of charcoal from a prehistoric hearth, or an oyster shell or bone from some ancient kitchen midden. If the carbon compound is more than 20,000 years old, however, the probable error in the dating is increased. The factor that limits the accuracy of the measurements is the ever present background radiation, which can never be screened out entirely. This radiation varies greatly from time to time and from place to place, and the radioactivity of the C^{14} must always be listened to over a variable amount of background noise. But in spite of all the limitations of the technique, it has shown its ability to date archaeological artifacts and anthropological specimens and, in so doing, has answered a great many important questions that otherwise could not have been answered. Thus atomic physics, with an assist from botany, has given a new and most important technique to anthropology and archaeology.

More than thirty centuries have passed in developing the science of biology. The building blocks have come from many lands and have been of many kinds. Some were the product of philosophical study, some came from the data supplied by simple observation or from the comparison of many different kinds of observation. Some

of the building material, that might otherwise have been lost, was obtained from carefully planned experiments. But the structure itself—the organized science of biology—was built by a logical interpretation and integration of all the raw material that was pertinent and that could be incorporated into an organized system.

As we have shown, biology, or rather the unorganized mass of knowledge that gradually achieved an orderly consistency and developed ultimately into the science of biology, came into being slowly and very imperfectly. It arose as a necessary adjunct to the evolution of our species because, if our ancestors were to succeed in their struggle for existence and survive long enough to leave offspring, they had to eat at appropriate intervals. To do this, they had to identify the plants they could consume with safety as well as those that would poison them. Thus, from sheer necessity, they became empirical botanists. And before they could supplement their always scanty diet with the concentrated proteins of meat, they had to learn something about the local animals and their habits. Obviously, all those who hunt or fish successfully have to know some zoology. Later on, when our predecessors learned to plant and harvest crops, they needed a reliable science of agriculture. Thus, our species needed a considerable knowledge of plants and animals merely to subsist; but this was not the only factor that forced them into becoming at least amateur biologists.

They also had ailments. The ills their flesh was heir to very naturally made them seek remedies. When they were in dire straits, which was often, they would try almost anything. At one time or another, they undoubtedly swallowed everything that they could get down their gullets and this, of course, led to discoveries. And so a primitive folk medicine came into being. Some prehistoric trepanned skulls which they left behind show that they also experimented on themselves. When they learned to investigate their own bodies and recognize their own organs, anatomy was born.

From such beginnings as these—from a primitive agriculture and from a highly fanciful medicine—grew the organized science of biology. Incidentally, medicine and agriculture are still the fields where the practical applications of biology pay the greatest dividends and where biology makes its more direct contributions to human welfare.

But biology is more than a mere system of useful techniques. It is now a science in the broadest sense of the term. Biologists have

long been seeking out new knowledge and seeking it for its own sake. And out of the great store of information that biologists have deposited in the archives of their science, new and basic concepts emerge from time to time. Some of these have been so drastic that they have changed our earlier orientations, both toward ourselves and toward the universe in which we live. Today, many of our more fundamental concepts are founded on biological data, or have originated in biological theory. No philosophy that concerns itself with human affairs can be complete, of course, if it ignores the biological nature of man, and no science of society can be valid if it evades the variables that characterize the creatures who, collectively, form the society.

During the centuries, biologists have focused their attention on many problems of many kinds—on problems that were trivial and on problems that were crucial for our understanding. Perhaps the most important of all problems, for us at least, are those concerned with life. What is life? How did it arise? What made it evolve? What will living things be like in the future? Where do novelties come from and how are they inherited? What are the physiological processes that make life possible? Such questions as these and many more are now being investigated intensively. Recent advances have made it possible to study some of these problems, even on the molecular level. Of course, many biologists are concerned with research problems of less general interest—but their work also is valuable. Often, from out of the discovery and recording of details trivial in themselves, come clues that lead to major advances.

Biology is now in one of its happy phases, but its future is not free from danger. Perhaps the greatest hazard lies in its very opulence. It has already grown far beyond the point where any biologist can master it, and the biologists have been compelled to take the only possible course of action. They have divided up their task and have limited their individual responsibilities. This has enabled them to collect biological data with unprecedented speed, but it has also increased the discrepancy between the biologists and their discipline. Biology continues to make great progress, but the progress is purchased at a considerable price. In order to master even a sub-section of the science—in order to specialize—the biologists have to concentrate their interests and their attention. And this means that they have to acquiesce in a constantly growing ignorance, an ignorance that increases at the same rate that the sum

total of biological information increases. It might well be that this newer ignorance will be the limiting factor in the growth of science, the factor that will change its exponential growth curve into one that is sigmoid.

Our species is subject to fashions in intellectual interests as well as to fashions in clothes. No one who has read extensively in the history of biology can fail to notice how frequently biologists have shifted their attention from one aspect of the science to another, only to return to the earlier fashion a generation or so later. Such behavior, of course, is not to be condemned. To change one's activity and follow a promising lead is obviously a sensible course of action, especially if there has been a "breakthrough" that can be exploited. Such behavior in the past has done little harm. The abandoned field has merely been "put on ice" or—more accurately—it has been "embalmed" in our libraries. But today, the problem of retrieval has become more serious and much harder to solve. Now, our scientific publications, because of their sheer volume, have become exceptionally difficult to catalogue and almost impossible to index. Any detailed history of biology will have to record instance after instance where crucial information has been buried for years, and has been disinterred only after it has been discovered independently and for a second time.

Another and more serious problem arises as the result of this necessary fragmentation of our science into specialized fields of research. It becomes more and more difficult to view the science as a whole. The interrelationship between the several parts may remain hidden, sometimes for years. Thus the data needed for a major advance may remain unassembled and the advance itself postponed. Often the specialists in one field have no knowledge of what they may find in another. They may not even know that the data they need exist. The historians of biology can cite instance after instance where a major advance has followed the integration of information that had existed for a considerable period but only as scattered and unorganized fragments.

It is certainly no accident that the great scientific progress being made today is being made in those fields where the sciences are able to make contact—in such fields as biophysics and biochemistry, physical chemistry, and mathematical physics. But even these biparental fields are now in the process of becoming specialties in their own right.

Fortunately, scientists rarely have jurisdictional disputes. They are by nature generous. Almost without exception, specialists give freely of their knowledge, their time, and their energy. Sometimes it is difficult to stop them. While it is becoming more difficult for specialists in widely separated fields to make intellectual contact—and sometimes they fail altogether—their failure is never due to a lack of effort. And whenever they succeed, we are almost certain to experience a new and spectacular advance. Today, many of the leading biologists are also at home in chemistry or in physics. The recent *rapprochement* between the biological and the physical sciences has already led to spectacular progress but, unfortunately, the biological and social sciences have still to make a workable intellectual contact. This, we hope, is a task that will be undertaken in the immediate future.

In thirty centuries, biology has spread its wings far indeed; from an incoherent succession of diffident attempts at fathoming life, it has grown into an organic whole which tries to probe and to understand all the vital processes. It is conscious of the limitations of its means; it looks toward an ideal, knowing full-well that the ideal is unattainable; yet it explores, and it will always continue to explore, because the urge to explore seems to be inherent in man.

Bibliography

I. SECONDARY SOURCES

ANDERSON, EDGAR. *Plants, Man and Life*. Boston: Little, Brown & Co., 1952.

ARBER, A. *Herbals, Their Origin and Evolution*. Cambridge: University Press, 1938.

BODENHEIMER, F. S. *The History of Biology: An Introduction*. London: Wm. Dawson & Sons, Ltd., 1958.

BRUNEL, JULES. "Antibiosis from Pasteur to Fleming," *Contrib. d'Inst. botan. Univ. Montreal*, No. 67 (1951).

BURCKHARDT, R., and ERHARD, H. *Geschichte der Zoologie und ihrer Wissenschaftlichen Probleme*. Leipzig: Goeschen, 1921.

CARRIER, LYMAN. *Beginnings of Agriculture in America*. New York: McGraw-Hill Book Co., Inc., 1923.

CHOULANT, LUDWIG. *History and Bibliography of Anatomic Illustration*. Chicago: University of Chicago Press, 1920.

CLODD, EDWARD. *Pioneers of Evolution from Thales to Huxley*. New York: 1897.

COCKAYNE, OSWALD. *Leechdoms, Wortcunning and Starcraft of Early England*. London: 1864.

COLE, F. J. *Early Theories of Sexual Generation*. Oxford: The Clarendon Press, 1930.

———. *A History of Comparative Anatomy from Aristotle to the Eighteenth Century*. London: Macmillan and Co., Ltd., 1944.

CRABB, A. R. *The Hybrid-Corn Makers*. New Brunswick, N. J.: Rutgers University Press, 1947.

CURWEN, E. CECIL, and GUDMUND HATT. *Plough and Pasture: The Early History of Farming*. New York: Abelard-Schuman, Ltd., 1953.

DUNN, L. C. (ed.). *Genetics in the 20th Century*. New York: The Macmillan Co., 1951.

DUPREE, A. HUNTER. *Asa Gray (1810–1888)*. Cambridge: Harvard University Press, 1959.

EAST, E. M., and D. F. JONES. *Inbreeding and Outbreeding: Their Genetic and Sociological Significance*. Philadelphia: J. B. Lippincott Co., 1919.

EICKSTEDT, EGON FRHR. VON. Geschichte der anthropologischen Namengebung und Klassifikation," *Zeitschrift f. Rassenkunde* 5:209–282, 6:35–210 (1937).

EISELEY, LOREN. "Charles Darwin, Edward Blythe, and the theory of natural selection," *Proc. Am. Phil. Soc.* 103:94–158 (1959).

EMMART, EMILY WALCOTT. *The Badianus Manuscript: An Aztec Herbal of 1552*. Baltimore: Johns Hopkins Press, 1940.

FLEMMING, W. "Kurze historische Uebersicht der Literatur über Zelltheilung" (in *Zellsubstanz, Kern und Zelltheilung*, pp. 389–400). Leipzig: 1882.

FLORKIN, M. (ed.). *Aspects of the Origin of Life*. New York: Pergamon Press, 1960.

FOCKE, W. O. *Die Pflanzenmischlinge*. Berlin: 1881.

Frazer, Sir James George. *The New Golden Bough.* New York: Criterion Books, Inc., 1959.

Frisk, Gosta. *A Middle English Translation of the Macer Floridus de Veribus Herbarum.* Stockholm: Almqvist and Wiksell, 1949.

Gabriel, M. G., and S. Fogel (eds.). *Great Experiments in Biology.* Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1955.

Gärtner, C. F. von. *Bastarderzeugung,* etc. Stuttgart: 1848.

Gandz, Solomon. "Artificial fertilization of date-palms in Palestine and Arabia," *Isis* 23:245–250 (1935).

Gaskin, Elizabeth B. "Why was Mendel's work ignored?" *Jour. Hist. of Ideas* 20:60–84 (1959).

Glass, Bentley. "Maupertuis and the beginnings of genetics," *Quart. Rev. Biol.* 22:196–210 (1947).

Goddard, D. R., and W. D. Bonner. "Cellular respiration," (in *Plant Physiology,* ed. by F. C. Steward, Vol. IA, pp. 209–312), New York: Academic Press, Inc., 1960.

Goodfield, G. J. *The Growth of Scientific Physiology.* London: Hutchinson & Co., Ltd., 1960.

Gowen, John W. (ed.). *Heterosis.* Ames, Iowa: Iowa State University Press, 1952.

Green, J. R. *A History of Botany.* Oxford: Clarendon Press, 1909.

Greene, Edward Lee. "Landmarks of botanical history," *Smithsonian Miscellaneous Collections,* Vol. 54 (1909).

Hagberg, Knut. *Carl Linnaeus.* London: Jonathan Cape, Ltd., 1952.

Harvey-Gibson, R. J. *Outlines of the History of Botany.* London: Adam & Charles Black, Ltd., 1919.

Herrell, W. E. *Penicillin and Other Antibiotic Agents.* Philadelphia: W. B. Saunders Company, 1946.

Iltis, Hugo. *Life of Mendel.* New York: W. W. Norton and Company, Inc., 1932.

Irving, G. W., and H. T. Herrick. *Antibiotics.* New York: Chemical Publishing Company, Inc., 1949.

Jacob, François, and E. L. Wollman. *Sexuality and the Genetics of Bacteria.* New York: Academic Press, Inc., 1961.

Judd, John W. *The Coming of Evolution.* Cambridge: University Press, 1912.

Karsten, Mia C. *The Old Company's Garden at the Cape, and its Superintendents.* Cape Town: Maskew Miller, Ltd., 1951.

Kramer, S. N. "Sumerian 'Farmer's Almanac.'" *Scientific American* 185:(11):54–55 (1951).

Larsen, Esther. "Pehr Kalm's description of maize," *Agric. Hist.* 9:98–117 (1935).

Laufer, Berthold. "The American plant migration," *Scientific Monthly* 28:239–251 (1929).

———. "The American plant migration—Part I: the potato," *Field Museum of Natural History Anthropological Series* 28:(1):1–132 (1938).

Locy, W. A. *Biology and Its Makers.* New York: Holt, Rinehart & Winston, Inc., 1908.

———. *Growth of Biology,* etc. New York: Holt, Rinehart & Winston, Inc., 1925.

Luria, G. E. *General Virology.* New York: John Wiley & Sons, Inc., 1953.

Lurie, Edward. *Louis Agassiz: A Life in Science.* Chicago: University of Chicago Press, 1960.

McCollum, E. V. *A History of Nutrition.* Boston: Houghton Mifflin Company, 1957.

Mangelsdorf, P. C. "The origin and evolution of maize," *Advances in Genetics* (New York), 1:161–209 (1947).

———, and R. G. Reeves. "The origin of Indian corn and its relatives," *Texas Agric. Exp. Sta. Bull.,* No. 574 (1939).

Mark, E. L. "Maturation, Fecundation and Segmentation of Limax campestris, Binney," *Bull. Mus. Comp. Zool. Harvard Univ.,* 6:173–620; general bibliography, 244–511 (1881).

MERRILL, ELMER DREW. "Domestic plants in relation to the diffusion of culture," *The Botanical Review* 4:1–20 (1938).

MEYER, E. H. F. *Geschichte der Botanik.* Königsburg: 1854–1857.

MEYERHOF, MAX. "Ibn an-Nafis . . . and his theory of the lesser circulation," *Isis* 23:100–120 (1935).

NÄF, WARNER, and MATHOUS GABTHULER. *Walahfrid Strabo: Hortulus vom Gartenbau.* St. Gallen: H. Tschudy, 1942.

NEEDHAM, JOSEPH. *A History of Embryology.* New York: Abelard-Schuman, Ltd., 1959.

NORDENSKIÖLD, ERIK. *A History of Biology.* New York: Tudor Publishing Company, 1935.

PETERS, J. A. (ed.). *Classic Papers in Genetics.* Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1959.

PUNNETT, R. C. "Early days of genetics," *Heredity* 4:121–133 (1950).

ROBERTS, H. F. *Plant Hybridization Before Mendel.* Princeton, N. J.: Princeton University Press, 1929.

SACHAU, EDWARD. *Alberuni's India.* London: 1887.

SACHS, JULIUS VON. *History of Botany.* Oxford: 1890.

SARTON, GEORGE. *An Introduction to the History of Science* (3 vols. in 5). Baltimore: The Williams & Wilkins Co., 1927, 1931, 1947, 1948.

———. "The discovery of the mammalian egg and the foundation of modern embryology," *Isis* 16:315–330 (1931).

———. *A History of Science* (2 vols.). Cambridge: Harvard University Press, 1952, 1959.

SCHIEMANN, ELISABETH. *Entstehung der Kulturpflanzen.* Berlin: Gebrüder Borntraeger, 1932.

SCHNEBEL, M. *Die Landwissenschaft im alten Aegypten.* Munich: 1925.

SHARP, L. W. *An Introduction to Cytology* (Historical Sketch, pp. 422–448). New York: McGraw-Hill Book Co., Inc., 1934.

SINGER, CHARLES. *A History of Biology.* New York: Abelard-Schuman, Ltd., 1950.

SINGLETON, W. R. "Early research in maize genetics," *Jour. Heredity* 26:49–59 (1935).

SIRKS, M. J. *Indisch Natuuronderzoek* (Amsterdam Koloniaal Instituut). Amsterdam: 1915.

———. *De Ontwikkeling der Biologie.* Gorinchem: J. Noorduyn en Zoon N. U., 1947.

———. "The earliest illustrations of chromosomes," *Genetica,* 26:65–76 (1952).

———. "Historical notes: Leeuwenhoek, Lamarck, Haeckel, Darwin, Boveri," *Genetica* 30:292, 391–92 (1959); 31:240, 420, 448 (1960).

STRAUSS, BERNARD S. *An Outline of Chemical Genetics.* Philadelphia: 1960.

TOLKOWSKY, S. *Hesperides: A History of the Culture and Use of Citrus Fruits.* Staples and Staples, Westminster, 1938.

VAVILOV, N. J. "The origin, variation, immunity and breeding of cultivated plants," tr. by K. Starr Chester, *Chronica Botanica* 13:1–364 (1951).

VERRILL, A. HYATT. *Foods America Gave the World.* New York: L. C. Page & Company, Inc., 1937.

WILSON, E. B. *The Cell in Development and Inheritance.* New York: 1896.

ZIRKLE, CONWAY. *The Beginnings of Plant Hybridization.* Philadelphia: University of Pennsylvania Press, 1935.

———. "Natural selection before the 'Origin of Species'," *Proc. Am. Phil. Soc.* 84:71–124 (1941).

———. "The early history of the idea of the inheritance of acquired characters and of pangenesis," *Trans. Am. Phil. Soc.* 35:91–151 (1946).

———. "Gregor Mendel and his precursors," *Isis* 42:97–104 (1951).

———. *Evolution, Marxian Biology and the Social Scene.* Philadelphia: University of Pennsylvania Press, 1959.

———. "Species before Darwin," *Proc. Am. Phil. Soc.* 103:636–644 (1959).

II. PRIMARY SOURCES

AL-'ABBAS AL-RASULI (*see* Sarton, 1948, p. 1,637). ABD-AL-LATIF: *Relation de l'Egypt*, etc. (tr. by de Sacy), Paris, 1810. ADANSON, MICHAEL: *Familles des Plantes*, Paris, 1763. AELIANOS, CLAUDIOS: *De Natura Animalium*, Paris, 1858. AGARDH, C. A.: Über die Anatomie und den Kreislauf der Charen," *Nov. Acta. Misc. Cur. C. L. C.*, 13:113–162 (1826). AGASSIZ, LOUIS: *Essay on Classification*, London, 1859. ST. ALBERTUS MAGNUS: *De Animalibus*, Paris, 1890–1899; *De Vegetabilibus et Plantis*, Paris, 1890–1899. ALDROVANDI, ULYSSES: *De Animalibus, Insectis* (Lib VII), Bononiae, 1602; *De Piscibus*, Bononiae, 1613; *De Quadrupedibus Solidipedibus*, Bononiae, 1616; *De Quadruped, Digitatis Viviparis*, Bononiae, 1637; *Ornithologia*, Bononiae, 1634; *Serpentum et Draconum Historia*, Bononiae, 1639; *Dendrologiae*, Bononiae, 1668. D'ALÉCHAMPS, JACQUES (Jacobus Dalechampius): *Historia Generalis Plantarum*, Lugduni Batavorum, 1586. ALPINI, PROSPERO: *Historiae Naturalis Aegypti sive de Plantis Aegypti*, Lugduni Batavorum, 1735. ALVERDES, FR.: "Der Kern in den Speicheldrüsen der Chironomus–Larve." *Archiv. Zellf.* 9:168–204 (1912). AMICI, G. B.: "Osservazioni sulla circuolazione del succhio nella Chara," *Mem. Nat. et Fis. Soc. Ital.* (Tom. 8, Vol. 2), 1818;*Osservazioni Microscopiche sopra varie Piante*, Modena, 1823. ANDERSON, E. C.: *Natural Radiocarbon*, Doctoral Thesis, University of Chicago, 1949. ANDERSON, E. C., W. F. LIBBY, S. WEISHOUSE, A. F. REID, A. D. KIRSCHENBAUM, and A. V. GROSSE: "Radiocarbon from Cosmic Radiation," *Science* 105:576–577. APULEIUS BARBARUS, or PLATONICUS (*see also* Cockayne). APULEIUS, LUCIUS: *The Golden Ass*, London, 1915. APULEIUS PLATONICUS: *Herbarium Apulei Platonici, Rome* (?), 1481. ARISTOTLE: *Generation of Animals* (tr. by A. L. Peck), Harvard University Press, Cambridge, 1943; *History of Animals* (tr. by d'Arcy Thompson), The Clarendon Press, Oxford, 1910; *Parts of Animals* (tr. by E. S. Forster), Harvard University Press, Cambridge, 1937. AL-ASMA'I: *The Book of Wild Animals* (ed. by D. H. Muller), Vienna, 1876; *The Book of the Horse* (ed. by A. Haffner), Vienna, 1895; *The Book of the Sheep* (ed. by A. Haffner), Vienna, 1896. AUERBACH, C., and J. M. ROBSON: "Chemical Production of Mutations," *Nature* 157:302 (1946). ST. AUGUSTINE: *On Marriage and Concupiscence* (tr. by Schaff), Buffalo, 1886.

BACHSTROM, JOHANNE: *Observationes circa Scorbutum*, Leiden, 1734. BACON, FRANCIS: *Sylva Sylvarum, or a Natural History*, London, 1683. BACON, ROGER: *Opus Majus* (tr. by R. B. Burke), University of Pennsylvania, Philadelphia, 1927. BAER, K. E. VON: *De Ovi Mammalium et Hominis Genesi*, Leipzig, 1827; *Über Entwicklungsgesichte der Tiere*, Königsberg, 1828. BAGLIVUS, GEORGIUS: *An Account of the Breeding of Worms in Human Bodies*, London, 1701. BALBIANI, E. G.: "Sur la structure du noyan des cellules salivaires chez les larves de Chironomus," *Zool. Anz.*, 4:637–641, 662–666, 1881. BALD: *The Leech Book of* (*see* Cockayne). BARTHOLIN, THOMAS: *De Lacteis Thoracicis*, Copenhagen, 1632; *Opuscula Nova de Lacteis*, Copenhagen, 1670. BARTHOLOMEW THE ENGLISHMAN: *All the Proprytees of Thynges*, Westminster, 1495. BARTRAM, WILLIAM: *Travels through North and South Carolina, Georgia, Florida, etc.*, Philadelphia, 1791. DE BARY, H. A.: *Vergleichende Anatomie der Phanerogamen und Farne*, Paris, 1877; *Comparative Anatomy of the Vegetative Organs of the Phanerogams and Ferns*, Oxford, 1884. ST. BASIL: *The Hexameron*, New York, 1895. BASSOS, CASSIANOS (Kassianos): *Geoponicorum sive De Re Rustica*, London, 1740. BATESON, W., E. R. SAUNDERS, and R. C. PUNNETT: "Experimental Studies in the Physiology of Heredity," Report III, Evolution Committee, Royal Society, 1906. BATESON, W., and R. C. PUNNETT: "On gametic series involving reduplication of certain terms," *Jour. Genetics* 1:293–302 (1911). BAUHIN, GASPARD: . . . *Enumeratio Plantarum*, Basel, 1596; *Theatri' Botanici* . . . , Frankfurt a.M., 1620. BAUR, E.: "Untersuchungen über das Wesen, die Entstehung und die Vererbung von Rassenunterschieden bei Antirrhinum majus," *Bibliotheca Gen.* 4:1–170 (1924). BEADLE, GEO. W.: "Teosinte and the Origin of Maize," *Jour. Heredity* 30:245–247 (1939). BEADLE, G. W., and E. L. TATUM: "Genetic Control of Biochemical Reactions in Neurospora," *Proc. Nat. Acad. Sci.* 27:499–506 (1941).

Beal, W. J.: "Indian corn," *Rept. Mich. State Bd. Agric.* 19:279–289 (1880). Belon, Pierre: *L'Histoire Naturelle des Estranges Poissons Marines*, Paris, 1551; *Les Observations de Plusieurs Singularitez et Choses Memorables, trouvees en Grece, Asie, Judee, Egypte, Arabie, etc.*, Paris, 1553; *La Nature et Diversités des Poissons*, Paris, 1555; *Portraits d'Oyseaux, Animaux, Serpens, Herbes, Arbres, Hommes et Femmes d' Arabie ed d' Egypt*, Paris, 1555; *De Arboribus, Coniferis, Resinferis, Aliis, etc.*, Paris, 1553. *Les Remonstrances sur la Default du Labor et Culturs des Plantes*, Paris, 1558. van Beneden, E.: "L'appareil sexuel femelle de l'Ascaride mégalocéphale," *Arch. de Biol.* 4:95–144 (1883). Berkeley, M. J.: *Introduction to Cryptogamic Botany*, London, 1857. Bernard, Claude: *Leçons sur les Phénomènes de la Vie Communs aux Animaux et aux Végétaux*, Paris, 1878. Berzelius, J. J.: *Lehrbuch der Chemie*, Dresden and Leipzig, 1835–1841. Beyerinck, M. W.: *Verzamelde Geschriften* (I–V, VI), Nyhoff, The Hague, 1921, 1940. Bichat, M. F. X.: *Recherches Physiologiques sur la Vie et la Mort*, Paris, 1800; *Anatomie Générale Appliquée à la Physiologie et à la Médicine*, Paris, 1801; *Traité des Membranes en Général*, Paris, 1802. Blair, Patrick: *Botanick Essays*, London, 1720; "Observations on the generation of plants," *Phil. Trans. Roy. Soc. London* 31:215–224 (1721). Blumenbach, J. F.: *The Anthropological Treatises of Blumenbach and Hunter* (tr. by T. Bendyshe), London, 1865. Blythe, Edward: "An attempt to classify the 'varieties' of animals, etc.," *Mag. Nat. Hist. N. S.* 8:40–53 (1835); "On the psychological distinction between man and all other animals," *Mag. Nat. Hist.* 2nd Ser.: 1:1–9, 77–85, 131–141 (1837). Bock, Jerome (Hieronymus Tragus): *New Kreutterbuch*, etc., Strassburg, 1539. du Bois-Reymond, Emil: "On the time required for the transmission of volition and sensation through the nerves" (*Croonian Lectures on Matter*, ed. by H. B. Jones), 1868. Bolsius, H.: "Sur la structure spiraleé ou discoide de l'element chromatique dans les glandes salivaires des larves de Chironomus," *La Cellule* 27:75–86. Bonnet, Charles: *Traité d'Insectologie*, Amsterdam, 1745; *Contemplation de la Nature*, Amsterdam, 1769; *Oeuvres d'Histoire Naturelle*, Neuchatel, 1781. Borries, B. von, and Rusha, E.: "Electron microscope and its uses," *Z. Ver. dent. Inf.* 70:519 (1935). Boussingault, J. B.: *Agronomie, Chimie Agricole et Physiologie*, Paris, 1860–1864. Boveri, T.: "Über Differenzierung der Zellkerne während der Furchung des Eies von *Ascaris megalocephala*," *Anat. Anz.* 2:688–693; *Ergebnisse über die Konstitution der chromatischen Substanz des Zellkernes*, Jena, 1904. Bradley, Richard: *New Improvements in Planting and Gardening*, London, 1717; *A Philosophical Account of the Works of Nature*, London, 1721; *A General Treatise of Husbandry and Gardening*, London, 1726. Braun, A.: *Über einige neue und weniger bekannte Krankheiten der Pflanzen*, Berlin, 1854. Bridges, Calvin B.: "Aberrations in Chromosomal Materials," *Eugenics, Genetics and the Family* 1:76–81 (1923); "Sex in relation to chromosomes and genes," *Amer. Nat.* 59:127–137 (1925); "Salivary chromosome maps," *Jour. Heredity* 26:60–64 (1935); "A revised map of the salivary gland X-chromosome," *Jour. Heredity* 29:11–13 (1938). Brodin, Gosta: *Agnus Castus: A Middle English Herbal*, A.-B. Lundequistska Bokhandeln, Upsala, 1950. Brongniart, A.: *Histoire des Végétaux Fossiles*, Paris, 1828–1835. Brown, Robert: "Observations on the organs and mode of fecundation in Orchideae and Asclepiadeae," *Trans. Linn. Soc.* 16:685–745 (1833). Brown, William L.: "Numbers and distribution of chromosome knobs in United States maize," *Genetics* 34:524–536 (1949). Brunfels, Otto von: *Herbarum Vivae Eicones*, etc., Argentorati, 1530. Buchner, E.: "Alkoholische Gährung ohne Hefezellen," *Ber. Deuts. Chem. Gesell.* 30:117–124 (1897). Bütschli, O.: ". . . . die ersten Entwickelungsvorgänge im befruchteten Ei von Hematoden und Schnecken," *Zeitschr. Wiss. Zool.* 25:201, 426 (1875). Buffon, Georges Louis Leclerc de: *Histoire Naturelle*, Paris, 1749–1804. Buonanni, Filippo: *Micrographia Curiosa*, Rome, 1691. Burnet, James (Lord Monboddo): *On the Origin and Progress of Language*, Edinburgh, 1773–1792. Busch, H.: "Calculation of a path of cathode rays in an axially symmetrical electromagnetic field," *Ann. Physik.* 81:974 (1926). Byrd, Wm.: *Wm. Byrd's Natural History of Virginia or the Newly Discovered Eden* (ed. and tr. by Beatty and Mulloy), Dietz Press, Richmond, Va., 1940.

Caius, John: *De Rariorum Animalium et Stirpium Historia,* London, 1570. Camerarius, R. J.: *De Sexu Plantarum Epistola* (Ost. Klas. exak. Wiss.), Leipzig, 1899. Camper, Pierre: *Oeuvres,* Paris, 1803. de Candolle, A. L. P. P.: *Géographie Botanique Raisonnée,* Paris, 1855; *Origin of Cultivated Plants* (reprint of 2nd. edition, 1886), Hafner Publishing Co., New York, 1959. Carrichter, Bartholomaeus: *Kräutterbuch,* Strassburg, 1575. Carus, C. G.: *Grundzüge der vergleichenden Anatomie,* Dresden, 1828; *Natur und Idee,* Vienna, 1861. Catesby, Mark: *Natural History of Carolina, Florida and the Bahama Islands,* London, 1731–1743. Cato, Marcus Porcius: *De Re Rustica,* Venice, 1514. Celsus, Aurelius Cornelius: *De Medicina Libri VIII* (text by L. Targa), Patavia, 1769. Cesalpino, Andrea: *De Plantis Libri XVI,* Florentae, 1583; *Quaestionum Peripateticarum,* Avignon, 1588. Chambers, Robert: *Vestiges of the Natural History of Creation,* London, 1844. Chu Hsiao (*see* Sarton, 1948, p. 1644). Clayton II, John (*see* Gronovius). Cleland, Ralph E.: "The Reduction Division in the Pollen Mother Cell of Oenothera Franciscana," *Amer. Jour. Bot.* 9:391–413 (1922). Coeiter, Volcherus (Volcker Koyter): *Externarum et Internarum Principalium Humani Corporis, etc.,* Noribergae, 1573. Cohn, F.: "Nachträge zur Naturgeschichte des *Protococcus nivalis,*" *Nova. Acta. Acad. Caes. Leop. Carol. Nat. Cur. Bonn.* 22:605–764 (1850). Columbo, Realdo: *De Re Anatomica,* Venice, 1559. Columella, Lucius Junius Moderatus: *Res Rustica* (tr. by M. C. Curtius), London, 1745. Cope, E. D.: "The Vertebrata of the Cretacious Formations of the West," U.S. Dept. of the Interior, *Geological and Geographical Survey of the Territories,* Vol. II, 1873. Cordus, Valerius: *Annotationes in Pedacii Dioscorides . . . de Materia Medica,* Argentorati, 1561. Correns, C.: "G. Mendels Regel über das Verhalten der Nachkommenschaft der Rassenbastarde," *Ber. d. deuts. bot. Gesell.* 18:158–168 (1900). Corti, B.: "Sur la circulation d'un fluide, découvert en diverses plantes," *Obs. Phys. Hist. Nat.* 8:232–294 (1776). Crescenzi, Piero de (Petrus de Crescentus) (*see also* Peter of Crescenzi): *Opus Ruralium Commodorum.* Bononiensis, 1471. Cusa, Nicolaus (N. Khrypffs): *D. Nicolai de Cusa . . . Opera.* Basel, 1565. Cushman, Samuel: *Ohio Poultry Journal.* 2:185–191 (1893). Cuvier, Georges: *Tableau élémentaire de l'histoire naturelle des animaux,* Paris, 1798; *Leçons d'Anatomie Comparée,* Paris, 1800, 1805; *Ossemens Fossiles,* Paris, 1812; *Le Régne Animal Distribué d'après son Organisation,* Paris, 1816; *Recherches sur les Ossemens Fossiles,* Paris, 1825. Cuvier, G., et M. de Saint-Agy: *Histoire des Sciences Naturelles, etc.,* Paris, 1841–1845.

Dalton, John: "Extraordinary facts relating to the vision of colours," *Mem. Lit. Phil. Soc. Manchester* 5:28–45 (1789). Al-Damiri (*see* Sarton, 1948, p. 1639). Danthine, Hélene: *Le Palmier-Dattier et les Arbres Sacrés dans l'Inconographie de L'Asie Occidentale Ancienne,* Librairie Orientaliste, Paris, 1937. Darwin, Chas.: "I. On the natural means of selection; on the comparison of domestic races and true species. II. Abstract of a letter from Chas. Darwin to Professor Asa Gray," *Jour. Linn. Soc. (Zool)* 3:46–53 (1858); *The Origin of Species,* London, 1859; *The Variation of Animals and Plants under Domestication,* New York, 1868; *The Descent of Man,* London, 1871; *The Effects of Cross and Self Fertilization in the Vegetable Kingdom,* London, 1876; *The Autobiography of Charles Darwin and Selected Letters,* New York, 1958. Darwin, Erasmus: *Zoonomia, or the Laws of Organic Life,* London, 1794–1796; *Temple of Nature,* London, 1803; *The Botanic Garden,* New York, 1807. Davenport, C. B.: "Review of Guaita's experiments in breeding mice," *Biol. Bull.* 2:121–128 (1901). Demerec, M.: "Origin of bacterial resistance to antibiotics," *Jour. Bact.* 56:63–74 (1948). Descartes, René: *Discourse on Method,* Leiden, 1637; *Traité de l'Homme,* Paris, 1664. Diderot, Dennis: *Lettre sur les Aveugles,* Paris, 1749. Digby, Sir Kenelm: *The Immortality of Reasonable Souls,* London, 1645. Dioskorides, Pedanios: *De Materia Medica Libri Quinque,* Leipzig, 1829; *The Greek Herbal of Dioscorides* (ed. by R. W. T. Gunther), New York: Hafner Publishing Company, Inc., 1959. Dobbs, Arthur: "Concerning bees and their method of gathering wax and honey," *Phil. Trans. Roy. Soc. London* 46:536–

549 (1750). Dobzhansky, Th.: "Genetical and cytological proof of translocation involving the third and fourth chromosomes of D. *melanogaster*.," *Biol. Zentb.* 49.408–419 (1929). Dodoens, Rembert: *Cruydeboeck*, Antwerpen, 1554; *Florum, et Coronariarum Odoratorumque, etc.*, Antwerp, 1568. Doncaster, L., and G. H. Raynor: "On breeding experiments with Lepidoptera," *Proc. Zool. Soc. London* 125–133 (1906). Douglass, William: *A Summary Historical and Political of the First Planting . . . of the British Settlements in N. America*, London, 1760. Drummond, J. C.: "Note on the role of the antiscorbutic factor in nutrition," *Biochem. Jour.* 13:77 (1919). Drummond, J. C., and Anne Wilbraham: *The Englishman's Food*, Jonathan Cape, Ltd., London, 1939. Dubois, Eugene: *Pithecanthropus Erectus, etc.*, Batavia, 1894. Duchesne, E.: *Contribution a l'étude de la concurrence vitale chez les microorganismes: antagonisme entre les moisissures et les microbes* (Thesis), Lyon, 1897. Dudley, Paul: "Observation on some of the plants in New England, etc.," *Phil. Trans. Roy. Soc. London* 33:194–200 (1724). Dujardin, F.: "Sur les organismes inférieurs," *Ann. Sci. Nat. Zool. II:* 3:343–377 (1835); *Histoire Naturelle des Infusoires*, Paris, 1841. Dutrochet, R. J. H.: *Recherches Anatomiques et Physiologiques sur la Structure Intime des Animaux et des Végétaux et sur leur Motilité*, Paris, 1824. Dzierzon, Johann: *J. Eichstadt Bienenzeitung* 1:113 (1845); *Der Bienenfreund aus Schlesien*, Brieg, 1856.

East, E. M.: "Inbreeding in corn," *Conn. Agric. Sta. Bull.* 159:419–428 (1908); "A Mendelian interpretation of variation that is continuous," *Amer. Nat.* 44:65–82 (1910); "Genetic aspects of certain problems of evolution," *Amer. Nat.* 70:143–158 (1936). de l'Ecluse, Charles (Carolus Clusius): *Rariorum Aliquot Stirpium per Hispanias Observatarum Historia*, Antwerpiae, 1570; *Rariorum Aliquot Stirpium, per Pannoniam, Austriam, et Vicinas Historia*, Antwerpiae, 1583; *Curae Postiores, etc.*, Antwerpiae, 1611. Ehrenberg, C. G.: *Die Infusions-thierchen als Vollkommene Organismen*, Leipzig, 1833. Eichler, A. W.: *Blütendiagramme*, Leipzig, 1875–1878. Eijkmann, C.: "Ein Versuch zur Bekämpfung des Beri-Beri," *Virchow Archiv. Path. Anat. u. Physiol.* 149:187–194 (1897). Eimer, G. H. Th.: *Entstehung des Arten*, Jena, 1888–1891. Emerson, R. A., and E. M. East: "The inheritance of quantitative characters in maize," *Neb. Ag. Ex. Sta. Res. Bull.* 2 (1913). Empedocles: Fragments translated by W. E. Leonard, Chicago, 1908. *The Encyclopedia of Islam.* Erhard, Hubert: "Über den Aufbau der Speicheldrüsenkerne der Chironomuslarve," *Arch. Mik. Anat.* 76:114–124 (1910). Esenbeck, C. G. D. Nees von: *Die allgemeine Formenlehre der Natur als Vorschule der Naturgeschichte*, Breslau, 1852. Evans, H. M., and K. S. Bishop: "On the existence of a hitherto unrecognized dietary factor essential for reproduction," *Science* 56:650–651 (1922).

Fabricius, Hieronymus (Aquapendente): *Opera Omnia Anatomica et Physiologica*, Lipsiae, 1687. Fairchild, Thomas: *The City Gardener*, London, 1722. Fallopii, Gabrielis: *Observationes Anatomicae*, Cologne, 1562. Farmer, J. B.: "Studies in Hepaticeae: On Pallavicinia decipiens," *Ann. Bot.* 8:35–52 (1894). Faussek, W.: "Zur Frage über den Bau des Zellkernes in den Speicheldrusen der larve von Chironomus," *Archiv. Mik. Anat.* 82:39–60 (1913). Ferrand, Gabriel: *Relations de Voyages et Textes Géographiques Arabes, Persans et Turcs*, Paris, 1913. Feulgen, R., and H. Rossenbeck: "Mikroskopisch–chemischer Nachweis einer Nucleinsäure, usw.," *Zeitsch. Physiol. Chem.* 135:203–248 (1924). Feulgen, R., and K. Voit: "Über den mechanismus der Nuclealfarbung, usw.," *Zeitsch. Physiol. Chemie.* 135:249–252; 136:57–61 (1924); "Über die für die Nuclealfarbung und Nuclealreaktion verantwortlich zu machenden Gruppen," *Zeitschr. Physiol. Chemie.* 137:272–286 (1924). Fisher, R. A.: "The correlation between relatives on the supposition of Mendelian inheritance," *Trans. Roy. Soc. Edinburgh* 52:399–433 (1918). Fleming, Alexander: "On the antibacterial action of cultures of a Penicillium," *Brit. Jour. Ex. Path.* 10:226–236 (1929). Flemming, Walter: "Über das Verhalteng des Kerns bei der Zellteilung," *Virchows Archiv.*, p. 77 (1880); *Zellsubstanz, Kern und Zellteilung*, Leipzig, 1882. Frampton, John: *Joyfull Newes out of the Newe Found Worlde*, London, 1577. Franckel-Conrat, H.: "The chemical basis of the infectivity of tobacco mosaic virus and other plant viruses" (in *The Viruses*, Vol I.,

pp. 429–457; ed. by Burnet Stanley), Academic Press, Inc., New York, 1959. FRANKEL-CONRAT H., and R. C. WILLIAMS: "Reconstruction of active tobacco mosaic virus from its inactive protein and nucleic acid components," *Proc. Nat. Acad. Sci.* 41:690–698 (1955). FRIEDRICH II: *De Arte Venandi cum Avibus*, Aedibus Insulae, Lipsiae, 1942; *Art of Falconry* (tr. by C. A. Wood), Charles T. Branford Co., Publishers, Newton Centre, Mass., 1955. FUCHS, LEONHARD: *De Historia Stirpium*, Basileae, 1542.

GAGER, C. S.: "Some physiological effects of radium rays," *Amer. Nat.* 42:761–778 (1908). GALEN: *Opera Omnia*, Leipzig, 1821–1833. GALTON, FRANCIS: *Hereditary Genius, etc.*, London, 1869; *Inquiries into Human Faculty and its Development*, London, 1883; *Natural Inheritance*, London, 1889. *Gart der Gesundheit*, Mainz, 1485. GASSENDI, PIERRE: *Opera*, Lyon, 1658. GEOFFROY ST. HILAIRE, ISIDORE: "Influence du monde ambiant pour modifier les formes animales," *Mem. Acad. Sci.* 12:79 (1833). GERARD, JOHN: *The Herball or Generall Historie of Plantes*, London, 1597. GESNER, KONRAD: *Historia Plantarum et Vires ex Dioscoride, Paulo Aegineta, etc.*, Paris, 1541; *Historia Animalium*, Zürich, 1551–1558. GIARD, ALFRED: *Controverses Transformistes*, Paris, 1904. GOEBEL, KARL VON: *Organographie der Pflanzen*, Jena, 1898. GOETHE, J. W. VON: *Versuch die Metamorphose der Pflanzen zu erklären*, Gotha, 1790; *Werke*, Wien, 1816–1818. GOSIO, B.: "Zersetzungen zuckerhaltigen Nahrmateriales durch den Vibriocholerae asiaticae," *Koch. Archiv. Hyg.* 22:1–27 (1895); "Ricerche batteriologiche e chimiche sulle alterazione del mais," *Riv. Iziene Sanit. Pub.* 7:825–849, 961–981 (1896). GOSS, JOHN: "On the variation in the colour of peas, occasioned by cross-impregnation," *Trans. Hort. Soc. London* 4:234–235 (1824). DE GRAAFF, REINER: *De Mulierum Organis Generationi Inservientibus*, Leiden, 1672. GRATIA, A., and S. DATH: "Moisissures et microbes bactériophages," *C. R. d. Soc. Biol. d. Paris* 92:461–462 (1925). GRAY, ASA: Articles Written by Asa Gray and Published in *Memoirs of the American Academy of Arts and Sciences* (N. S. Vol. 6, Ch. 8–11), Boston, 1859. ST. GREGORY OF CONSTANTINOPLE: *Scolia* (Migne Collection, Ser. 2, II: 911), Paris, 1868–1888. GREW, NEHEMIAH: *The Anatomy of Vegetables Begun*, London, 1672; *The Anatomy of Plants*, London, 1682. GRONOVIUS, J. F.: *Flora Virginica.* Lugduni Batavorum, 1739, 1762.

HAECKEL, ERNST: *Generelle Morphologie der Organismen*, Jena, 1866; *Natürliche Schöpfungsgeschichte*, Berlin, 1868. HALE, SIR MATTHEW: *The Primitive Origination of Mankind*, London, 1677. HALES, STEPHEN: *Vegetable Staticks*, London, 1727. HALLER, ALBERT VON: *Elementa Physiologiae Corporis Humani*, Lausannae, 1757–1766. HAN CH'AN-CHIH: "Han Yen-Chih's Chu Lu" (tr. by M. J. Hagerty), *T'oung Pao* 22:63–96 (1923). HARVEY, WILLIAM: *De Motu Cordis*, Frankfurt a. M., 1628; *Exercitationes de Generatione Animalium*, London, 1651. HEDWIG, J.: *Theoria Generationis . . . plantarum cryptogamicarun*, Leipzig, 1798. HEITZ, E., and H. BAUR: "Beweise für die Chromosomennatur der Kernschleifen in den Knäuelkeanen *von Bibio hortulanus*," *Zeit. Zellf. Mik. Anat.* 17:67–82 (1933). HELMHOLTZ, H. V.: *Lehre von Tonempfindungen*, Braunschweig, 1862; *Physiologische Optik* (in *Allgemeine Encyklopädie der Physik*, ed. by G. Karsten), Leipzig, 1866. VAN HELMONT, J. B.: *Ortus Medicinae*, Amsterdam, 1648. HENKING, H. VON: "Untersuchungen über die ersten Entwicklungs vorgänge in den Eiern des Insekten," *Zeitschr. f. Wiss. Zool.* 91:685–736 (1891). HENSCHEL, A. W. E. T.: *Von der Sexualität der Pflanzen*, Breslau, 1820. HERACLEITOS: *Heraklitos von Ephesos* (in Greek and German, ed. by H. Diels), Berlin, 1901. *Herbarius in Latino*, Mainz, 1484. *Herbarius zu Teutsch*, Augspurg, 1485. HERODOTOS: *Herodotus with an English Translation by A. D. Dodley*, New York, 1921. HERSHEY, A. D., and B. ROTMAN: "Genetic recombination between host-range and plage-type mutants of bacteriophage in single bacterial cells," *Genetics* 34:44–71 (1949). HERTWIG, O.: "Bildung, Befuchtung und Teilung des thierischen Eies." *Morph. Jahrb.* 1:347–434 (1875). VAN HERWERDEN, M. A.: "Über die Kernstructur in den Speicheldüsen der Chironomus Larve," *Anat. Anzeiger.* 36:193–207 (1910). HESIOD: *Works and Days* (tr. by Banks), London, 1856. HILDEBRAND, F.: "Einige Experimente und Beobachtungen,

usw.," *Bot. Zeitschr.* 26:321–328 (1868). HILL, R.: "Oxygen production from isolated chloroplasts," *Proc. Roy. Soc. London B.* 127:192–201 (1939). HIPPOCRATES: *The Genuine Works of Hippocrates* (tr. by Francis Adams), New York, 1886. HOBBES, THOMAS: *Leviathan*, London, 1651. HOFFMANN, H.: *Untersuchungen zur Bestimnung des Werthes von Species und Varietät*, Giessen, 1869. HOFMEISTER, W.: "Über die Entwickelung des Pollens," *Botanische Zeitung* 6:425–434, 649–658, 670–674; *Vergleichende Untersuchungen der Keimung höherer Kryptogamen*, Leipzig, 1851; *Die Lehre von der Pflanzenzelle*, Leipzig, 1867. HOLBACH, P. H. T.: *The System of Nature* (Edition 1, published in 1770), New York, 1836. HOOKE, ROBERT: *Micrographia*, London, 1667. *Hortus Sanitatis*, Mainz, 1491; *The Noble Lyfe and Natures of Man, of Bestes Serpentys, fowles and fisshes y† be Most Knowen.* Antwerpt, 1521 (facsimile reproduction), Bernard Quaritch, Ltd., London, 1954. HUANG TAO-PUI (*see* Sarton, 1947, p. 832). HUBER, FRANÇOIS: *Nouvelles Observations sur les Abeilles*, Genève, 1792. HUMBOLDT, A. VON, and A. BONPLAND: *Géographie des Plantes*, Paris, 1805. HUME, DAVID: *An Abstract of a Treatise of Human Nature*, Cambridge University Press, London, 1938. HUNTER, JOHN: *The Anthropological Treatises of Blumenbach and Hunter* (tr. by T. Bendyshe), London, 1865. HU SSU-HUI (*see* Sarton, 1947, p. 905). HUTCHINSON, J. B.: *The Evolution of Gossypium*, Oxford University Press, Inc., New York, 1947. HUTTON, JAMES: *Theory of the Earth*, Edinburgh, 1795. HUXLEY, T. H.: *Collected Essays*, New York, 1896–1902.

IBN-AL-'AWWAM: *Libro de Agricultura* (tr. by Banqueri), Madrid, 1802. IBN-SINA: *Canon Medicinae*, Lovuain, 1658. INGENHOUSZ, JAN: *Experiments upon Vegetables, Discovering their Great Power of Purifying the Common Air in the Sunshine, etc.*, London, 1779; *An Essay on the Food of Plants*, London, 1796. ST. ISIDORE OF SEVILLE: *Opera Omnia*, Paris, 1862.

JAGER, GUSTAV: *Lehrbuch der Allgemeinan Zoologie*, Leipzig, 1878. AL-JAHIZ: "Book of Animals (tr. by M. Palacios)," *Isis* 14:20–54 (1930). JANSEN, B. C. F., and W. F. DONATH: "On the isolation of the anti-beriberi vitamin," *Proc. Kon. Akad. Weten. Amsterdam* 29:1390–1400 (1926). JOHANNSEN, W.: *Über Erblichkerit in Populationen und in Reinen Linien*, Jena, 1903. JOHNSON, S. W.: *How Crops Grow*, New York, 1891. JOHNSTON, JOHANNES: *Historia Naturalis Animalium*, Amstelodami, 1657. JONES, D. F.: "Dominance of linked factors as a means of accounting for heterosis," *Genetics* 2:466–479 (1917); "The effects of inbreeding and cross breeding in development," *Conn. Agric. Sta. Bull.* 207, 1918. DE JUSSIEU, A. L.: *Genera Plantarum*, Paris, 1789.

KALM, PER: *Beschreibung der Reise die er nach dem Noerdlichen Amerika*, Gottingen, 1754–1756; *The America of 1750: Peter Kalm's Travels in North America* (ed. by A. B. Benson), New York, 1937; (*see* Larsen, 1935). KANT, IMMANUEL: *Bestimmung des Begriffs einer Menschenrace*, Leipzig, 1839. KAUFMANN, B. P.: "Chromosome structures in Drosophila," *Amer. Nat.* 65:555–558 (1931). KEILIN, D.: "On Cytochrome, a respiratory pigment, common to animals, yeast and higher plants," *Proc. Roy. Soc. London B.* 98:312–329 (1925). KLIPPART, J. H.: "An essay on the origin, growth, diseases, varieties, etc. of the wheat plant," *Ann. Rep., Ohio State Bd. Agric.* 12:562–816 (1858). KNIGHT, T. A.: "An account of some experiments on the fecundation of vegetables," *Phil. Trans. Roy. Soc. London* 89:195–204 (1799); "Some remarks on the supposed influence of the pollen in cross breeding," *Trans. Hort. Soc. London* 5:377–380 (1824). KOCH, ROBERT: *On the Investigation of Pathogenic Organisms*, London, 1886. KÖLLIKER, R. A. VON: *Beiträge zur Kentnis der Geschlechtsverhältnisse und der Samenflüssigkeit wirbelloser Tiere*, Berlin, 1841; *Entwicklungsgeschichte der Cephalopoden*, Zürich, 1885; *Erinnerungen aus meinem Leben*, Leipzig, 1899. KOELREUTER, J. G.: *Vorläufige Nachricht von einigen das Geschlecht der Pflanzen betreffenden Versuchen und Beobachtungen*, Leipzig, 1766. KOERNICKE, M.: "Über die Wirkung von Röntgenstrahlen auf die Keimung und das Wachstum," *Ber. d. Deutsch. Bat. Ges.* 22:148–155 (1904); "Die Wirkung der Radiumstrahlen auf die Keimung und das Wachstum," *Ber. d. Deutsch. Bot. Ges.* 22:155–166 (1904). KONRAD VON MEGENBERG: *Das Puch der Natur*, Stuttgart, 1861. KORSCHELT, E.: Über

die eigentümliche Bildung in den Zellkernen der Speicheldrüse von *Chironomus plumosus*," Zool. *Anz.* 7:189–194, 221–225, 241 (1884). KOSTOFF, DONCHO: "Discoid Structure of the Spireme," *Jour. Hered.* 21:323–324 (1930). KRAUSE, F.: "Examination of viruses by the electron microscope," *Naturwissenschaften* 26:122 (1938). KREBS, H. A., and W. A. JOHNSTON: "The role of citric acid in intermediate metabolism in animal tissues," *Enzymologia* 4:148–156 (1937).

LACTANTIUS: On the Workmanship of God (Ante-Nicene Fathers, Vol. 7), Buffalo, 1886. DE LAMARCK, JEAN BAPTISTE PIERRE ANTOINE (de Monet): *Système des animaux sans vertèbres*, Paris, 1801; *Zoological Philosophy*, London, 1914. LAWRENCE, W.: *Lectures on the Natural History of Man*, London, 1819. LAWSON, JOHN: *A Voyage to Carolina*, London, 1709; *A History of Carolina*, London, 1714. LAYARD, E. L.: "On the Ornithology of Ceylon," *Ann. Mag. Nat. Hist. 2nd Ser.* 14:57–64 (1854). LEDERBERG, J.: "Gene recombination and linked segregation in *Escherichia coli*," *Genetics* 32:505–525 (1947). LEDERBERG, J., and E. L. TATUM: "Gene recombination in *Escherchia coli*," *Nature* 158:558 (1947). LEDERMÜLLER, M. F.: *Mikroskopische Gemüts und Augenergotzungen*, Nuremberg, 1750–1762. LEEUWENHOEK, ANTON VON: *Opera Omnia*, Lugduni Batavorum, 1715–1722. LEIBNITZ, G. W.: *Oeuvres . . . nouvelle édition, etc.*, Paris, 1846. LEO AFRICANUS, JOANNES: *The History and Description of Africa*, London, 1896. LEONARDO DA VINCI: *Leonardo da Vinci on the Human Body* (ed. by C. D. O'Malley and J. B. de C. M. Saunders), New York, 1952; *The Notebooks of Leonardo da Vinci* (ed. by E. MacCurdy), New York, 1958. LEVY, LUDWIG: "Über Farbstoffe in den Muskeln," *Zeits. f. Physiol. Chem.* 13:309–325 (1889). LEYDIG, FR.: *Untersuchungen Zur Anatomie und Histologie der Tiere*, Bonn, 1883. LIBBY, W. F.: *Radiocarbon Dating*, University of Chicago Press, Chicago, 1952; "Radiocarbon Dating," *Endeavour* 13:5–16 (1954). LIEBIG, J. VON: *Complete Works on Chemistry*, Philadelphia, 1839. LINDLEY, J., and W. HUTTON: *Fossil Flora of Great Britain*, London, 1831–1837. LINNAEUS, CAROLUS: *Systema Natura*, Lugduni Batavorum, 1735; *Bibliotheca Botanica*, Amsterdam, 1736; *Fundamenta Botanica*, Amsterdam, 1736; *Musea Cliffortiana*, Lugduni Batavorum, 1736; *Critica Botanica*, Lugduni Batavorum, 1737; *Flora Lapponica*, Amsterdam, 1737; *Genera Plantarum*, Lugduni *Batavorum*, 1737; *Hortus Cliffortianus*, Amsterdam, 1737; *Viridarium Cliffortianum*, Amsterdam, 1737; *Classes Plantarum*, Lugduni Batavorum, 1738; *Ichtologia*, Leiden, 1738; *Flora Suecica*, Stockholm, 1745; *Flora Zeylanica*, Amsterdam, 1747; *Hortus Upsaliensis*, Stockholm, 1748; *Materia Medica Regni Vegetabilis*, Leipzig, 1748; *Materia Medica Regni Animalis*, Uppsala, 1750; *Philosophia Botanica*, Stockholm, 1751; *Species Plantarum*, Halmiae, 1753; "De Sexu Plantarum," *Amoen. Acad.* 10:100–131 (1760). VAN LINSCHOTEN, JAN HUYGENS: *Itinerarium ofte Schipvaert naer Oost ofte Portugaels Indien*, Amsterdam, 1596. LOEB, J.: *The Dynamics of Living Matter*, New York, 1906. LONITZER, ADAM (Lonicerus): *Kreuterbuch, etc.*, Frankfurt a. M., 1557. LORT, MICHAEL: "An account of a remarkable imperfection of sight," *Phil. Trans. Roy. Soc. London.* 68:612–614 (1778). LOTSY, J. P.: *Evolution by means of Hybridization*, The Hague, 1916. LUCRETIUS (Titus Lucretius Carus): *De Natura Rerum* (tr. by Rouse), J. M. Dent & Sons, Ltd., London, 1916. LUDWIG, K. F. W.: *Lehrbuch der Physiologie*, Heidelberg, 1852. LU MING-SHAN (*see* Sarton, 1947, p. 832). LURIA, S. E., and R. DULBECCO: "Genetic recombination leading to production of active bacteriophage from ultraviolet inactivated bacteriophage particles," *Genetics* 34:93–125 (1949). D'LUZZI, MONDINO: *Anatomia Mundina*, Strassburg, 1513. LYELL, CHARLES: *Principles of Geology* (3 vols.), London, 1830–1833; *The Geological Evidence of the Antiquity of Man*, London, 1863. LYONET, PIERRE: *Traité Anatomique de la Chenille*. The Hague, 1746.

MCCALLUM, E. V., N. SIMMONS, and W. PITZ.: "The nature of the deficiencies of the oat kernel,"*Jour. Biol. Chem.* 29:341–354 (1917). MCCLUNG, C. E.: "Notes on the accessory chromosome," *Anat. Anz.* 20:220–226 (1901); "The accessory chromosome," *Biol. Bull.* 3:43–84 (1902). MCCLURE, G. W.: "Corn Crossing," *Ill. Agric. Ex. Sta. Res. Bull.* 2:82–101 (1892). MACMUNN, C. A.: "Researches on myo-

haematin and histohaematins," *Phil. Trans. Roy Soc. London* 177:267–298 (1886); "Über das myohaematin," *Zeit. f. Physiol. Chem.* 14:328–329 (1890). *Macer Floridus* (*see* Frisk). MAIMONIDES, MOSES: *Guide to the Perplexed*, E. P. Dutton & Co., Inc., New York, 1904. MALTHUS, THOMAS: *Essay on the Principles of Population*, London, 1826. MANGELSDORF, P. C., and REEVES, R. G.: "A Trigeneric Hybrid of Zea, Tripsacum, and Euchlaena," *Jour. Heredity* 26:129–140. MARIOTTE, E.: *Sur le Sujet des Plantes*, Paris, 1679. MARSH, O. C.: *Introduction and Succession of Vertebrate Life in America*, New Haven, 1877. MARTON, L.: "A new electron microscope," *Phys. Rev.* 58:57 (1940); "The electron microscope: first attempt at its application to biology," *Ann. Bull. Soc. Roy.: Sci. Med.-Nat. Bruxelles.* 92:106 (1934). AL-MASUDI: "Meadows of gold and mines of precious stones" (in *El-Masudi's Historical Encyclopedia*, tr. by A. Sprenger), London, 1841. MATHER, COTTON: *Religio Philosophica, or the Christian Philosopher*, London, 1721. MATTHAEUS, SYLVATICUS: *Pandectae*, Venice, 1480. MATTHEW, PATRICK: *Naval Timber and Arboriculture*, London, 1831. MATTIOLI, PIERANDREA: *Di Pedacio Dioscoride Anazarbeo . . . Historia*, Venetia, 1544. MAUPAS, E.: "Le Rajeunissement Karyogamigue chez les cilíes," *Arch. de Zoology.* 2:149–517 (1889). (MAUPERTUIS, P. L. M. DE: *Oeuvres*, Lyons, 1756. MAXIMUS OF TYRE: *The Dissertations of Maxmus Tyrius* (tr. by T. Taylor), 1804. MECKEL, J. F.: *Beyträge zur vergleichenden Anatomie*, Leipzig, 1808–1812; "Complément des observations sur le *Marchantia polymorpha*," *Mem. Acad. Sci. Paris* 13:375–436 (1935). *Medicinische Ephemeriden*, Chemnitz, 1793. MENDEL, GREGOR: "Versuche über Pflanzenhybriden," *Verh. Naturf. Ges. in Brunn* 4:1–47 (1866). METTRIE, J. OFFRAY DE LA: *Man a Machine*, London, 1750; *Oeuvres Philosophiques*, Amsterdam, 1753; *Oeuvres Philosophiques*, Berlin, 1774. MEYEN, F. J. F.: "Über die eigenthümliche Saftbewegung in den Zellen der Pflanzen," *Nova. Acta. Misc. Cur. C. L. C.* 13:841–874 (1827). MIESCHER, F.: "Phosphorvergiftung bei Hünhern," *Zeit. f. Physiol. Chem.* 4:441–460 (1871). MILLER, PHILIP: *The Gardener's and Florist's Dictionary*, London, 1724. MILLER, S. L.: "A production of amino acid under possible primitive earth conditions," *Science* 117:528–529 (1953). MIRBEL, C. F.: *Exposition et Défense de ma Théorie d' Organisation Végétale*, La Haye, 1808; "Complément des observations sur le *Marchantia polymorpha*," *Mem. Acad. Sci. Paris* 13:375–436 (1935). VON MOHL, H.: *Grundzüge der Anatomie und Physiologie der vegitabilischen Zelle*, Braunschweig, 1851. MONARDES, NICOLAS: *Historia Medicinal . . . de Neustras Indias Occidentales*, Sevilla, 1569. MONTGOMERY, T. H.: "A study of the chromosomes of the germ cells of metazoa," *Trans. Amer. Phil. Soc.* 20:154–236 (1901). MORGAN, T. H.: "Chromosomes and heredity," *Amer. Nat.* 44:449–496 (1910); "Sex-linked inheritance in Drosophila," *Science* 32:120–122 (1910); "The method of inheritance of two sex-limited characters in the same animal," *Proc. Soc. Exp. Biol. Med.* 8:17–19 (1910); "Random segregation versus coupling in Mendelian segregation," *Science* 34:384 (1911); "The application of the conception of pure lines to sex-limited inheritance and to sexual dimorphism," *Amer. Nat.* 45:65–89 (1911); "An attempt to analyze the constitution of the chromosomes on the basis of sex-linked inheritance," *Jour. Exp. Zool.* 11:365–414 (1911); MÜLLER, HERMANN: "La Fécondation des fleurs par les insectes," *Archiv. Sci. Phys. Nat.* 48:289–304 (1873); *Fertilization of Flowers*, London, 1883. MÜLLER, JOHANNES: *Handbuch der Physiologie*, Coblenz, 1833. MÜLLER, O. F.: *Animalcula Infusioria*, Copenhagen, 1786. MULDER, G. J.: *The Chemistry of Vegetable and Animal Physiology*, Edinburgh and London, 1849. MULLER, H. J.: "Artificial transmutation of the gene," *Science* 66:84–87 (1927); "The production of mutations by X-rays," *Proc. Nat. Acad. Sci.* 14:714–726 (1928). MULLER, H. J., and T. S. PAINTER: "Parallel cytology and genetics of induced translocations and deletions in Drosophila," *Jour. Heredity* 20:287–298 (1929).

NAEGELI, CARL: *Zur Entwicklungsgeschichte des Pollens*, Zürich, 1842; *Mechanisch-physiologische theorie der Abstammungslehre. usw.*, Munich, 1884. IBN-AN-NAFIS (*see* Meyerhoff, 1935). NAUDIN, C. V.: "Considérations Philosophiques sur l'Espèce et la Variété," *Revue Horticole Sér.* 4:102–109 (1852); "Remarques au sujet du croisement supposé des variétés blanche et violette du Haricot d'Espagne,"

Bull. Soc. Bot. Paris 3:179–181 (1856); "Nouvelles recherches sur l'hybridité dans les végétaux," *Ann. Sci. Nat. Bot.* 4th Ser. 19:180–203 (1863). NAWASCHIN, S.: "Neue Beobachtungen über Befruchtung bei *Fritillaria tenella* und Lilium marfagon," *Bot. Zentralb* 77:62 (1899). NECKAM, ALEXANDER: *De Naturis Rerum*, London, 1863. NEEDHAM, J. TURBERVILLE: *An Account of Some New Microscopical Discoveries*, London, 1745. NEWELL, WILMON: "Inheritance in the honey bee," *Science* 41:218–219 (1915). NEWPORT, G.: "On the impregnation of the ovum in Amphibia, etc.," *Phil. Trans. Roy. Soc. London* 143:233–290; 144:229–244 (1853, 1854). NICANDER OF COLOPHON: *Theriaca et Alexipharmaca recensuit et emondavit, fragmenta collegit*, etc., Leipzig, 1856. NICHOLAS OF DAMASCUS: *De Plantis libri duo Aristoteli vulgo adscripti*, Leipzig, 1841. VAN NIEL, C. B.: *Photosynthesis in Bacteria* (in *Contributions to Marine Biology*, pp. 161–169), Stanford University Press, Stanford, California, 1929; "The bacterial photosyntheses and their meaning for the general problem of photosynthesis," *Adv. Enzymology* 1:263–328 (1941). NILLSON-EHLE, H.: "Einige Ergebnisse von Kreuzungen bei Hafer und Weizen," *Bot. Notiser*. 257–294 (1908). NIZAMI-I-ARUDI: "Four Discourses" (tr. by C. G. Browne), *Jour. Roy. Asiatic* Soc. (July and October, 1899). NUSSBAUM, M.: "Zur Differenzierung des Geschlechts im Tierreich," *Archiv. Mik. Anat.* 18:1–121 (1880).

DE L'OBEL, MATHIAS (Mathias Lobelius): *Plantarum seu Stirpium Historia*, Antwerp, 1576. DE L'OBEL, MATHIAS, and PIERRE PENA (Mathias Lobelius et Petrus Pena): *Stirpium Adversaria Nova*, London, 1570. OKEN, LORENZ: *Lehrbuch der Naturphilosophie*, Jena, 1809; *Allgemeine Naturgeschichte für aller Stande*, Stuttgart, 1841. OPARIN, A. I.: *Origin of Life on Earth*, Edinburgh and London, 1961. OPPIAN (Oppianos): *De Venatione* (Lib. IIII), *De Piscatu* (Lib. V), Lugduni Batavorum, 1597. D'ORTA, GARCIA: *Aromatum et Simplicium. . . . Indos Nascentium Historia*, Antwerpiae, 1567. OSBORN, H. F.: *From the Greeks to Darwin*, London, 1927. OTTO, JOHN C.: "An account of Haemorrhagic disposition existing in certain families," *Med. Repository* 6:1–4 (1803). OVERTON, E.: "On the reduction of the chromosomes in the nuclei of plants." *Ann. Bot.* 7:139–143 (1893). OVIEDO Y VALDEZ, GANZALO FERNANDEZ DE: *Historia Natural y General de las Indias*, Sevilla, 1535. OWEN, SIR RICHARD: *On Parthenogenesis: or procreating individuals from a single ovum*, London, 1849; *On the Anatomy of Vertebrates*, London, 1866–1868.

PAINTER, T. S.: "A new method for the study of chromosome re-arrangements and the plotting of chromosome maps," *Science* 78:585–586 (1933); "Salivary chromosomes and the attack on the gene," *Jour. Heredity* 25:465–476 (1934). PAINTER, T. S., and H. J. MULLER: "Parallel cytology and genetics of induced translocations and deletions in Drosophila," *Jour. Heredity* 20:287–298 (1929). PALLADIUS, RUTILIUS TAURUS AEMILIANUS: *De Agricultura* (tr. into Middle English and ed. by B. Lodge, published by the Early English Text Society), London, 1873–1879. PALMER, ROBERT BAUER: *Walahfridi Strabi Liber de Cultura Hortorum* (Thesis), University of Pennsylvania, Philadelphia, 1951. PANDER, C. H.: *Beiträge zur Entwicklungsgeschichte des Hübnchens im Eye*, Würzburg, 1817. PARACELSUS (Theophrastus Bombastus von Hohenheim): *Opera Omnia*, Geneva, 1658. PARKINSON, JOHN: *Theatrum Botanicum or The Theater of Plants*, London, 1640. PASTEUR, LOUIS: "Mémoire sur les corpuscules organisés, etc., *Ann. Sci. Nat. Zool.* 16:5–98 (1862); "Note sur un mémoire de M Liebig relatif aux fermentations," *C. R. Paris Acad. Sci.* 73:1419–1424 (1871). PAULEY, A.: *Darwinismus und Lamarckismus*, Munich, 1905. PAULMIER, F. C.: "The spermatogenesis of *Anasa tristis*," *Jour. Morph.* 15: Suppl. 223–272 (1899). PAYER, J. B.: *Traité d' Organogénie Végétale Comparée de la Fleur*, Paris, 1857. PEARSON, KARL: "On a generalized theory of alternative inheritance with special reference to Mendel's law," *Phil. Trans. Roy. Soc. London A.* 203:53–86 (1904). PEKELHARING, C. A.: "Over onze kennis van de waarde voerdingamiddelen uit chemische fabrieken," *Nederl. Tijdschr. Geneeskunde* 70:111–124 (1905). PEROTUS, NICOLAUS: *Cornucopia*, Basel, 1521. PERRAULT, CLAUDE: *Essais de la Physique*, Paris, 1680. PETER OF CRESCENZI: *New Feldt und Ackerbaw*, Strassburg, 1602. PHILES, MANUEL: *De Animalium Proprietate*, Trajecti

ad Rhenum, 1730. *Physiologus:* (The Old English Physiologus, ed. by A. S. Cook), Yale University Press, New Haven, 1921. PLINY (Gaius Plinius Secundus): *The Historie of the World* (tr. by Holland), London, 1601. PLINY THE YOUNGER: *Letters* (tr. by William Melmoth, revised by W. M. L. Hutchinson, 2 vols.), Loeb Classical Library, London, 1924–1927. PORTA, GIAMBATTISTA: *De Humani Physiognomonica,* Vici Aeguensis, 1586; *Phytognomonica,* Neapoli, 1588. PRICHARD, J. C.: *Researches into the Physical History of Man,* London, 1826. PRIESTLEY, JOSEPH: *Experiments and Observations on Different Kinds of Air,* London, 1774; "An account of persons who could not distinguish colours," *Phil. Trans. Roy. Soc. London* 67:262–263 (1777). PRINGSHEIM, N.: *Zur Kritik und Geschichte der Untersuchungen über das Algengeschlecht,* Berlin, 1856. PROVOST, L. J., et J. B. DUMAS: "Nouvelle théorie de la génération," *Ann. Sci. Nat. Bot.* 1:1–187; 2:100–127, 129–149 (1824); PRUESSNER, A. H.: "Date culture in ancient Babylonia," *Amer. Jour. Sem. Lit.* 36:213–232 (1920). PSELLOS, MICHAEL: *De Omnifaria Doctrina,* Hamburg, 1718. PUNNETT, R. C.: "Reduplication series in sweet peas, II," *Jour. Genetics* 6:185–193 (1916); "Linkage in the sweet pea (*Lathyrus odoratus*)," *Jour. Genetics* 13:101–124 (1923). PURKINJÉ, J. E.: "Über die Analogen in den Structur-Elementen des tierischen und pflanzlichen Organismus," *Breslau, Schles. Gesell. Ubersicht,* p. 81–82 (1839).

AL-QAZWINI: *On the Marvels of the Countries* (in Gabriel Ferrand's *Relations de Voyages et Textes Géographiques Arabees, Persans et Turcs*), Paris, 1913.

RABL, C.: "Über Zellteilung," *Morphol. Jahrb.* 10:214–330 (1885). RAUBER, A.: "Neue Grundlegungen zur Kentnis der Zelle," *Morph. Jahrb.* 8:233–338 (1883). RAUWOLF, LEONHART: *Aigentliche Beschreibung der Raiss, etc.,* Laugingen, 1583. RAY, JOHN: *Table of Plants,* Amsterdam, 1669; *Methodus Plantarum Nova,* London, 1682; *Historia Piscium,* London, 1686; *Historia Plantarum,* London, 1686; *Synopsis Animalium Quadrupedum,* London, 1693; *Synopsis of Birds,* London, 1713; *The Wisdom of God Manifested in the Works of Creation,* London, 1743. RÉAUMUR, RENE ANTOINE FERCHAULT DE: *Mémoires pour Servir à l'Histoire des Insectes,* Paris, 1734–1742; *Art de Faire Éclorre . . . des Oiseau Domestiques,* Paris, 1749. REDI, FRANCESCO: *Esperienze Intorno alla Generazione degl' Insetti,* Florence, 1668. REMAK, R.: "Anatomische Beobachtungen über das Gehirn, etc." *Müller Archiv.* 506–522 (1841). RHEEDE TOT DRAAKENSTEIN, H. A. VAN: *Hortus Indicus Malabaricus,* Amsterdam, 1678–1703. RÖNTGEN, W. C.: *On a New Kind of Rays* (in G. F. Barker's *Roentgen Rays*), New York, 1899. RÖSEL VON ROSENHOF, A. J.: *Monatlich . . . Insektenbelustigung,* Nuremberg, 1746–1761; *Historia Naturalis Ranarum,* Nuremberg, 1758. RONDELET, GUILLAUME: *De Piscibus Marinis,* Lugduni Batavorum, 1554. ROUX, WILHELM: *Der Kamp der Theile im Organismus,* Leipzig, 1881; *Über die Bedeutung der Kernteilungsfiguren,* Leipzig, 1883; *Einleitung zu den Beiträgen zur Entwicklungsmechanik des Embryo,* Vienna, 1885; *Entwicklungsmechanik der Organismen,* etc., Leipzig, 1895. RUBIN, S., M. RANDALL, M. KAMEN, and J. L. HYDE: "*Heavy oxygen* (O^{18}) as a tracer in the study of photosynthesis," *Jour. Am. Chem. Soc.* 63:877–879 (1941). RUDBECK, OLOF: *De Circulatione Sanguinis,* Uppsala, 1652; *Nova Exercitatio Anatomica,* Uppsala, 1653. RÜCKERT, J.: "Zur Befuchtung des Selachiereies," *Anat. Anz.* 6:308–322 (1892); "Zur Entwickelungsgeschichte des Ovarialeies bei Selachiern," *Anat. Anz.* 7:107–158 (1892). RUEL, JEAN (Johannes Ruellius): *De Natura Stirpium,* Paris, 1536. RUMPF, G. E. (Rumphius): *D'Amboinsche Rariteitkamer,* Amsterdam, 1705; *Herbarium Amboinense,* Amsterdam, 1741–1750. RUMPF, VINCENT: *De Hominibus Orbis Nostri Incolis.* Hamburg, 1721. RUSKA, E., and KNOLL, M.: "The magnetic concentrating electromagnet for fast electrons," *Zeit. Tech. Physik.* 12:389 (1931).

SACHS, JULIUS VON: *Vorlesungen über Pflanzenphysiologie,* Leipzig, 1882. SAGERET, A.: "Considérations sur la productions des hybrides," *Ann. Sci. Nat.* 1st Ser. 8:294–313 (1826). ST. GEORGE, LA VALLETTE: "Uber die Genese der Samenkorper," *Archiv. Mik. Anat.* 1:403–414 (1865). SANBORN, J. W.: "Indian Corn," *Rept. Maine Dept. Agric.* 33:54–121 (1890). DE SAUSSURE, N. TH.: *Recherches Chemiques*

sur la Végétation, Paris, 1804. SCHATZ, A., E. BUGEE, and S. A. WAKSMAN: "Streptomycin, a substance exhibiting antibiotic activity against positive and gram-negative bacteria," *Proc. Soc. Ex. Biol. Med.* 55:66–69 (1944). SCHEIL, V.: "De l'exploitation de dattiers dans l'ancienne Babylonia," *Revue d' Assyriologie* 10:1–9 (1913). SCHELVER, F. J.: *Kritik der Lehre von den Geschlechten der Pflanzen*, Heidelberg, 1812. SCHEUCHZER, J. J.: *Herbarium Diluvianum*, Tegusi, 1709. SCHIMPER, K. F.: *Beschreibung der Symphytum usw.*, Heidelberg, 1835. SCHLEIDEN, M. J.: "Beiträge zur Phytogenesis," *Arch. Anat. Phys. Wiss. Med.* 137–176 (1838). SCHOBER, A.: "Ein Versuch mit Röntgens'chen Strahlen auf Keimpflanzen," *Ber. d. Deutsch. Bot. Ges.* 14:108–110 (1896). SCHULTZE, MAX: "Uber Muskelkörperchen und das, was man eine Zelle zu nennen habe," *Archiv. Anat. Physiol. Wiss. Med.* 1–27 (1861). SCHWANN, THEODORE: *Mikroskopische Untersuchungen über die Übereinstimmung in der Structur und dem Wachstum der Thiere und Pflanzen*, Berlin, 1839. SCHWEIGGER-SEIDEL, O.: "Über die Samenkörperchen und ihre Entwickelung," *Archiv. Mik. Anat.* 1:309–335 (1865). SECKT, H.: "Über den Einfluss des X-strahlen auf den pflanzlichen Organismus," *Ber. d. Deutsch. Bot. Ges.* 20:87–93 (1902). SELDIN, M.: "Über die Wirkung des Röntgen-und Radiumstrahleen auf innere Organe und Gesamtorganismus der Tiere," *Fortschr d. Röntgenstr.* 7:322–339 (1904). SEMON, R.: *Das Problem der Vererbungsr erworbener Eigenschaften*, Leipzig, 1912. SENEBIER, JEAN: *Recherches sur l'Influence de la Lumière Solaire*, etc., Geneve, 1783. SERVET Y REVES, MIGUEL (Servetus): *Christianismi Restitutio*, Villanovarus, 1790; *De Trinitatis Erroribus*, Hagenau, 1531; *Michael Servetus, a Translation of his Geographical, Medical and Astrological Writings*, Philadelphia, 1953. SETON, ALEXANDER: "On the variation in the colour of peas from cross impregnations," *Trans. Hort. Soc. London.* 5:236 (1824). SHULL, G. H.: "The composition of a field of maize," *Rept. Am. Breeders Ass.* 4:296–301 (1908); "Inheritance of sex in Lychnis," *Bot. Gaz.* 49:110–125 (1910); "Reversible sex-mutants in *Lychnis dioica*," *Bot. Gaz.* 52:329–368 (1911); "Über die Vererbung der Blattfarbe bei Melandrium," *Ber. Deutsch. Bot. Ges.* 31:40–80 (1913). SLOANE, SIR HANS: *Catalogus Plantarum, etc.*, London, 1696; *Voyage to the Islands of Madeira, Barbados, etc.*, London, 1707–1725. SPALLANZANI, LAZZARO: *Saggio di Osservazioni Microscopiche Concernenti il Sistema delle Generazioni de Signori di Needham e Buffon*, Modena, 1766; *Sopra le Reproduzioni Animali*, Geneva, 1768; *Expériences pour Servir à Histoire des Animaux et des Plantes*, Geneva, 1785. SPEMANN, H.: *Embryonic Development and Induction*, Yale University Press, New Haven, 1938. SPENCER, HERBERT: *Social Statics*, London, 1851; "A New Theory of Population," *Westminster Reviews*, April, 1852; *Principles of Biology*, London, 1863, 1864. SPILLMAN, M. J.: "Spurious allelomorphism," *Amer. Nat.* 42:610–615 (1908). SPRENGEL, C. K.: *Das entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen*, Berlin, 1793. SPRENGEL, G.: *Historia Rei Herbariae*, Amsterdam, 1807–1808. STADLER, L. J.: "Genetic effects of X-rays in maize," *Proc. Nat. Acad. Sci.* 14:69–75 (1928). STANLEY, W. M.: "Isolation of a crystalline protein possessing the properties of tobacco-mosaic virus," *Science* 81:644–645 (1935). STENO, NICOLAUS: *The Prodromus to a Dissertation Concerning Solids Naturally Contained Within Solids* (tr. by H. O.), London, 1671. STRASBURGER, E.: *"Über Befruchtung und Zelltheilung*, Jena, 1877; *Neue Unterschungen uber den Befruchtungsvorgang über den Phanerogamen etc.*, Jena, 1884; "Sur la division des noyaux cellulaires, la division des cellules et la fécondation," *Jour. Bot. Paris* 2:81–91 (1888); "Über periodische Reduktion der Chromosomenzahl im Entwicklungsgange der Chromosomen," *Ann. Bot.* 8:281–316 (1894). STURLI, A.: "Über ein in Schimmelpilzen (*Penicillium glaucum*) vorkommendes Gift," *Wien. Klin. Wsch.* 21:711–714 (1908). STURTEVANT, A. H.: "The linear arrangement of six sex-linked factors in Drosophila," *Jour. Exp. Zool.* 14:43–59 (1913). SUTTON, W. S.: "The chromosomes in heredity," *Biol. Bull.* 4:231–251 (1903). SWAMMERDAM, JAN.: *Bijbel der Natuure*, Leiden, 1737. SYLVIUS, JACOBUS (Jacques Du Bois): *De Hominis Generatione, sive Fecunditatis et Sterilitatis Causis*, Geneva, 1530; *Vesali cuiusdam Caluminarum in Hippocratis et Galeni Rem Anatomicam*, Paris, 1551.

TABERNAEMONTANUS (Jakob Theodor von Berg-Zabern): *Neuw Krauterbuch*, Basel, 1588. TÄNZER, E.: "Die Zellkerne einiger Dipterenlarven und ihre Entwicklung," *Zeit. Wiss. Zool.* 119:114–153 (1921). TAMMES, TINE: "Mutation und Evolution," *Zeitschr. indukt. Abst.–u. Vererbungslehre* 36:417–426 (1925). TAYLOR, J. H., and S. H. TAYLOR: "The autoradiograph—a tool for cytogeneticists," *Jour. Heredity* 44:128–133 (1953). THEOPHRASTOS: *Graece and Latine Opera Omnia*, Lugduni Batavorum, 1613; *Inquiry into Plants* (tr. by Hort), Loeb Classical Library, London, 1916. ST. THOMAS AQUINAS: *Opera Omnia*, London, 1911–1917; *Summa Theologica*, Washbourne, London, 1912. THOMAS OF CANTIMPRÉ: *De Natura Rerum* (*see* Konrad von Megenberg). THURET, G.: "La Fécundation des Fucacées," *Ann. Sci. Nat. Bot.* 4:197–214 (1854); "La Fécundation des Fucacées et les anthéridies des Algues," *Ann. Sci. Nat. Bot.* 4:3:5–28 (1855). TOURNEFORT, J. P. DE: *Institutiones Rei Herbariae*, Paris, 1700–1703. TREMBLEY, ABRAHAM: *Mémoires pour Servir à l' Histoire d' un Genre de Polypes d' Eau Douce*, etc., Leiden, 1744. TREVIRANUS, L. C.: "Beobachtungen über die Bewegung des kornigen Wesens in einigen Conferven und einer Chara," *Beitr. Zur Naturkunde* 2:126–141 (1810); *Die Lehre vom Geschlechte der Pflanzen*, Bremen, 1822. TSERCHMAK, E. VON: "Über künstliche Kreuzung bei Pisum sativum," *Ber. d. deuts. bot. Gesell.* 18:232–239 (1900). TURNEISSES, LEONHARDT: *Historia, sive Descriptio Plantarum*, Berlin, 1578. TURNER, WILLIAM: *A New Herball*, London, 1551. TYNDALL, JOHN: "On the development of organisms in organic infusions," *Nature* 13:268–270 (1876). TYLOR, E. B.: "The winged figures of the Assyrian and other ancient monuments," *Proc. Soc. Bib. Archeology* 12:383–393 (1890). TYSON, EDWARD: *Orang-Outang sive Homo Sylvestris*, London, 1699.

UNGER, F.: *Beiträge zur Kenntniss der niedersten Algenformen*, Wein, 1854. UREY, H. C.: *The Planets, their Origin and Development*, Yale University Press, New Haven, 1952.

VALLISNIERI, ANTONIO: *Opere Diversi*, Venezia, 1715. VARRO: *De Re Rustica* (tr. by Storr-Best), London, 1912. VERGILIUS MARO, PUBLIUS: *Eclogues, Bucolics, or Pastorals of Virgil* (tr. by R. F. Rozds), Oxford University Press, New York, 1922; *The Georgics of Virgil* (tr. by C. D. Lewis), London, 1946. VERLOT, B.: *Sur la Production et la Fixation des Variétés dans les Plantes d' Ornament*, Paris, 1865. VESALIUS, ANDREAS: *De Humani Corporis Fabrica*, Basel, 1543. VICQ D' AZYR, F.: *Traité d' Anatomie et Physiologie*, Paris, 1786. VILMORIN, H.: "Sur la fécondation du mais," *Bull. Soc. Bot. France* 14:246–249 (1867). VINCENT OF BEAUVAIS: *Speculum Naturale*, Basel, 1481. VIRCHOW, RUDOLPH: "Cellular-Pathologie," *Arch. Path. Anat. Phys.* 8:1–39 (1855). DE VRIES, HUGO: "Das Spaltungsgesetz des Bastarde," *Ber. d. deuts. bot. Gesell.* 18:83–90 (1900).

WAHLBOM, J. S.: "Sponsalia Plantarum," *Amoen. Acad.*, Vol. I, 1746. WAKSMAN, S. A.: *Microbial Antagonism and Antibiotics Substances*, New York, 1945; *Soil Microbiology*, New York, 1952. WALAHFRID, STRABO: *De Cultura Hortorum* (tr. in English by R. S. Lambert), Wembly Hill, 1924; ——: *Hortulus Vom Gartenbau*, St. Gallen, 1942. WALDEYER, W.: "Über *Karyokinese und ihre Beziehungen zun den Befruchtungsvorgängen*," *Archiv. Mik. Anat.* 32:1–122 (1888). WALLACE, A. R.: "On the law which has regulated the introduction of new species," *Annals and Magazine of Natural History*, September, 1855; "On the tendency of varieties to depart indefinitely from the original type," *Jour. Linn. Soc. (Zool.)* 3:53–62 (1858); *My Life: A Record of Events and Opinions*, London, 1905. WANG CHEN (*see* Sarton, 1947, p. 830). WARBURG, OTTO: "The enzyme problem and biological oxydations," *Bull. Johns Hopkins Hospital* 46:341–358 (1930). WARMKE, H. E.: "Sex determination and sex balance in Melandrium," *Amer. Jour. Bot.* 33:648–660 (1946). WARMKE, H. E., and A. F. BLAKESLEE: "Sex mechanism in polyploids of Melandrium," *Science* 89:391–392 (1939); "The establishment of a 4N dioecious race in Melandrium," *Amer. Jour. Bot.* 27:751–752 (1940). WATSON, J. D., and F. H. C.

CRICK: "Molecular structure of nucleic acid," *Nature* 171:737–738 (1953); "The Structure of DNA," *Cold Spring Harbor Symposia on Quantitative Biology* 18-123–131 (1953). WEBBER, H. J.: "Xenia or the immediate effect of pollen in maize," *U. S. Dept. of Agriculture Div. Veg. Phys. Bull.* 22 (1900). WEISMANN, AUGUST: *Studies in the Theory of Descent*, London, 1880–1882; *Über die Zahl der Richtungskorper. usw.*, Jena, 1887; *The Germ Plasm*, New York, 1893. WELLS, W. C.: *Essays upon Double and Single Vision*, London, 1818. WENT, F. W.: *The Experimental Control of Plant Growth*, The Ronald Press Co. (A Chronica Botanica Publication), New York, 1957. WHERRY, E. T.: "Reflections on the origin of life," *Proc. Penn. Acad. Sci.* 10:12–15 (1936). WHITING, P. W.: "Sex determination in bees and wasps," *Jour. Heredity* 26:263–278 (1935); "Multiple alleles in sex determination of Habrobracon," *Jour. Morph.* 66:323–352 (1940). WHITING, P. W., and A. R. WHITING: "Diploid males from fertilized eggs in Hymenoptera," *Science* 62:437–438 (1925). WILSON, E. B.: "The chromosomes in relation to the determination of sex in insects," *Science* 20:500–502 (1905); "Studies on chromosomes III," *Jour. Ex. Zool* 3:1–40 (1906). WÖHLER, F.: *Grundriss der Chemie*, Berlin, 1833. WOLFF, C. F.: *Theoria Generationis*, Halle, 1759. WOTTON, EDWARD: *De Differentiis Animalum*, Paris, 1552. WRIGHT, SEWALL: "Evolution in Mendelian populations," *Genetics* 16:97–159 (1931). WYCKOFF, RALPH W. G.: *Electron Microscopy*, Interscience Publishers, Inc., New York, 1949.

YANOVSKY, ELIAS: "Food Plants of the North American Indians," *U. S. Dept. of Agriculture, Miscellaneous Publications, No. 237*, Washington, 1936. YULE, UDNY: "Mendel's laws and their probable relation to intra-racial heredity," *New Phytologist* 1:193–207, 223–238 (1902).

ZALUZIANSKY, ADAM: *Methodi Herbariae*, Pragae, 1592. ZELLER, E.: "Über die griechischen Vorgänger Darwins," *Abhandl. Berl. Akad. Wiss.* 111–124 (1878). ZINDER, N., and J. LEDERBERG: "Genetic exchange in Salmonella," *Jour. Bact.* 64:679–699 (1952).

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